# CHEMICAL ESSAYS;

BEING A
CONTINUATION
OF MY
*Reflections on Fixed Fire*,
WITH
OBSERVATIONS AND STRICTURES
UPON
*Drs.* PRIESTLEY'*s*, FORDYCE'*s*, PEARSON'*s*,
*and* BEDDOES'*s late Papers*
IN THE
PHLIOSOPHICAL TRANSACTIONS;
AND
AN ANSWER TO THE REVIEWERS.

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*BY ROBERT HARRINGTON, M. D.*

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*Magna vis eſt veritatis, et prævalebit.* CICERO.

——— nothing extenuate,
Nor ſet down ought in malice. SHAKESPEARE.

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[1793]

TO

# *Foreign Philoſophical Chemiſts.*

In a country ſuch as Britain, diſtinguiſhed for its liberality, ſenſe, and generoſity, and where a Society is eſtabliſhed by *Royal* munificence, for the encouragement of ſcience, it appears ſingular that a Briton ſhould have to ſeek patronage and protection from foreigners: but ſuch is the caſe.—I have called upon my countrymen, both collectively and individually, to give my doctrines a candid and fair diſcuſſion; but they have not hitherto had the liberality to do it. They, from their labours, have received great honours for ſuppoſed diſcoveries. But Philoſophy knows no partialities,—ſhe only venerates truth; and that man deſerves not the name of a philoſopher, who would ſkulk from inveſtigation. It is inveſtigation that I want; therefore I hope I ſhall receive it from you. In the great field of ſcience, each labourer is a citizen of the world, and his appeal is to it. If, upon a fair and public examination, my ſyſtem is found fallacious, I ſhall willingly give it up; for truth is my object.

My ſyſtem is a more extenſive one than has hitherto been given to the world; for it not only comprehends fire through all its ſituations in nature, but the important formation and purpoſes of air, and particularly its uſes in animal and vegetable life, beſides many other chemical phenomena; embracing all the experiments and phenomena of modern chemiſtry, and appealing to the late numerous experiments for its evidence.—Therefore I hope from you it will have a candid examination; and that honour will be your reward, at leaſt that pleaſure which proceeds from a liberal action.

I am, Gentlemen,

With the greateſt Reſpect,

Your moſt obedient humble Servant,

CARLISLE, July 10, 1793. } ROBERT HARRINGTON.

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Theſe ESSAYS would have been publiſhed in the ſpring, had I not been waiting for the next volume of *The Philoſophical Tranſactions*, in order that I might have made my obſervations upon any chemical papers it contained; as I ſuppoſe our chemical philoſophers ſtill continue to work in their unproductive mine:—but it is ſo long in being publiſhed, that I have not ſeen it.

Both Dr. Crawford and Mr. Keir have promiſed the Society an explanation of ſome experiments and phenomena, upon their principles, which they have not done. To what muſt we impute it? I ſhould ſuppoſe they find themſelves aground.

# CHEMICAL ESSAYS.

## A CONTINUATION OF REFLECTIONS UPON FIXED FIRE, THE FORMER PART BEING PUBLISHED IN THE GENTLEMAN'S MAGAZINE FOR 1792.

I SHALL now conſider the effervefcence of nitrous and pure airs. The phenomenon of nitrous air being united to pure air has greatly aſtoniſhed modern chemiſts. They decompound each other. When this is inveſtigated, it will, I believe, be found to proceed from the common principle of an acid body uniting to an alkaline one.

The only airs we are acquainted with that will unite and decompound each other, are alkaline and acid airs; viz. the vitriolic acid air, the aerial acid air, and the marine acid air; theſe unite with alkaline air, forming the vitriolic, the common, and the marine ammoniac. The nitrous air, which, agreeable to my doctrine, I ſuppoſe the nitrous acid united to phlogiſton, will not unite with alkaline air. The reaſon appears to be this; the nitrous acid has ſo ſtrong an attraction for its phlogiſton and aerial ſtate, that it cannot be decompounded. But if theſe two airs are united, not only comprehends fire through all its ſituations in nature, but the important formation and purpoſes of air, and particularly its uſes in animal and vegetable life, beſides many other chemical phenomena; embracing all the experiments and phenomena of modern chemiſtry, and appealing to the late numerous experiments for its evidence.— Therefore I hope from you it will have a candid examination; and that honour will be your reward, at leaſt that pleaſure which proceeds from a liberal action.

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But expoſe nitrous air to pure air, and they will produce the ſame effect, *i. e.* decompound each other; and we ſhall find, upon inveſtigation, that they do it upon the ſame principle as the acid and alkaline airs, uniting together. And this I ſhould ſuppoſe a leading argument to prove that pure air is formed of ſimilar conſtituent parts to alkaline air, as it ſhews a ſimilar efferveſcence when decompounded, and turns the nitrous air red, producing heat in the decompoſition.

There is no occaſion here to reſume the arguments tending to prove, that the fire which forms fixed air and water into pure air, is ſomething of a ſimilar concentration to the fire that compoſes alkalies; eſpecially as I have elſewhere proved, that the pure air formed from nitre conſiſts of the acid, alkali, and water aeralized together: and that the acid being phlogiſticated by being aeralized with the alkali, weakens its attraction for the alkali. This is ſeen in nitre, which, when long expoſed to heat, its acid will become ſo phlogiſticated, that the vegetable acid, (as Mr. Scheele found) will expel it from the alkali. Therefore the nitrous acid in the nitrous air will attack the alkali, and produce ſuch a fermentation as to decompound both airs. That this is owing to the acid in the nitrous air, is evident; for if it is neutralized ſo as not to affect the vegetable juices, it will not decompound pure air, as in nitrous dephlogiſticated air. And ſtill more particularly, if the pure nitrous vapour is united to alkaline air, or pure air, it will (as Dr. Prieſtley found) decompound them as quick as nitrous air.—See vol. III. p. 193.

But to know whether this theory, or that of Mr. Lavoiſier, is the true one, the beſt teſt would be accurately to examine the reſiduum. If, according to Mr. Lavoiſier, the nitrous acid is formed of pure air and nitrous air, we ſhould be able to form the pure nitrous acid. But, agreeably to my theory, the nitrous air is formed of the nitrous acid and plogiſton, and pure air of an acid and water united, with a concentration of fire ſimito an alkaline ſaturation; or, as when the latter is made from nitre with the real alkali, only volatilized, and by that means united with more looſe fire, and therefore much more eaſily ſet looſe as aerial fire.

Moreover, by accurately examining the reſiduum, which has not as yet been properly attended to, we ſhall know what hypotheſis we ought to adopt. The two airs having a ſtrong attraction for each other, produce the ſame phenomena; as for inſtance, if an acid air, the vitriolic acid air, and the alkaline air were united together: therefore uniting them in that proportion, ſo as to ſaturate each other, they ſhould form the pure nitrous acid. But they form a highly phlogiſticated nitrous acid, ſo highly, that if the decompoſition is made over water; the acid receiving water, (which I have fully proved to be the body that compoſes airs) will again produce nitrous air in the greateſt abundance, and the reſiduum itſelf will be highly plogiſticated.

Dr. Prieſtley ſays,—"The ſaturation of water "with nitrous acid from nitrous air, by means of "deplogiſticated air, makes a pleaſing experi"ment, on account of the great quantity of nitrous "air decompoſed by this means at one proceſs, "the quickneſs with which the decompoſition is "made, and the viſible effect of the ſudden im"pregnation on the water. For the ſurface of it "inſtantly becomes, as it were, oily, deſcending "in waves from the top to the bottom of the "water, while nitrous air iſſues from the bottom "and ſides of the veſſel; a moſt remarkable "phenomenon, of which a full account will be "given in a proper place."†

Now, will Mr. Lavoiſier grant, that upon the nitrous acid formed from the decompoſition of nitrous air, pure air is imbibed by the water, the water chemically attracting it, ſo as again to form nitrous air? If not ſo, how came it to be formed again? But we are ſtill aground, for the reſt of the acid is highly phlogiſticated. To this chemical philoſophers may anſwer, that the phlogiſton is owing to its not being fully ſaturated with pure air. But if there is a greater proportion of dephlogiſticated air than the nitrous acid can decompound, the phenomenon is the ſame.—Then what hinders the full ſaturation? Are we acquainted with any bodies which have a ſtrong affinity for each other, that will ruſh together with ſo ſtrong an attraction, but will perfectly ſaturate each other? Will alkalies or earths not ſaturate acids? Or take any chemical bodies you pleaſe. Why then can we make the pure nitrous acid; or how will the acid, when it has gotten its watery baſe again form freſh nitrous air?

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† Obſerv. and Exper. &c. vol. III. p. 164.

In my Thoughts on Air, publiſhed in 1785, I have, I think, ſatisfactorily proved, that nitrous air is formed of the nitrous acid, phlogiſton, and water aerilized by heat. Therefore, in the decompoſition or fermentation between the pure air and the nitrous air, the heat which formed the union between the water, and the phlogiſticated acid is ſet free, in conſequence of which the water is precipitated. But when it again meets with water and heat from the fermentation, nitrous air is again produced. However as we have already ſhewn in pure air, its fire is ſo looſely concentrated, that fermentation, combuſtion, or muſcular action can eaſily ſet it looſe as actual heat; its acid is precipitated in its fixed ſtate in this proceſs of the mixing pure air and nitrous air together, in the ſame manner as if done by combuſtion, forming the ſame acid as that with which it is united; as when phoſphorus, or ſulphur, is burned with it, they form the phoſphoric or ſulphurous acids.*

It ſhould not be matter of ſurpriſe, that two airs unite together and decompound each other of their

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* "This is Mr. Lavoiſier's grand argument for ſuppoſing, that the oxygen gas, with a great part of its caloric, is condenſed into the nitrous acid; or when the nitrous and oxygen gas decompound each other, that they will comparatively produce little heat. But the cauſe is this, in their condenſation, the greateſt part of their fixed fire or phlogiſton is not ſet looſe or free, but is condenſed with the airs.

"For, according to our theory, we have ſhewn that pure air muſt neceſſarily poſſeſs both fixed fire and an acid, in order to ſupport combuſtion. The ſame argument will take place when the factitious dephlogiſticated air, from the calx of metals and the nitrous acid are decompounded by the nitrous air: for the nitrous acid of the nitrous air attracts the fixed fire, which is neutralized or concentrated with the acid, water, and a ſmall part of the earth of the calx, which form oxygen gas, as having a ſtronger attraction for the fire than the airs acid have. Dephlogiſticated nitrous air, which is a compound of the nitrous acid and phlogiſton, more amply confirms this doctrine; for the nitrous air will not decompound it; its acid being equally as ſtrong, being the ſame acid, as that which the nitrous air poſſeſſes; *i. e.* being weakened their aerial form, ſince all acid and alkaline airs do it. We ſee that ſulphur, iron, and water will efferveſce and decompound their phlogiſton into actual fire.

But there is another aerial body, viz. the nitrous vapour, which will decompound nitrous air, and which their received chemical theories cannot account for.† "This vapour," ſays Dr. Prieſtley, vol. II. p. 169, "is the pure nitrous "*vapour of the acid*, which is inſtantly and wholly "imbibed by the water." Here it appears that the pure nitrous vapour will likewiſe decompound nitrous air. Hence, according to Mr. Lavoiſier's theory, the vapour, or the nitrous air, muſt either give or receive pure air from each other, which ever he chooſes to make it. But then, if it was the vapour that gave the pure air, it either ſhould (after the proceſs) have formed nitrous air or phlogiſticated air. But here is a loſs inſtead of a generation of airs; and only one-fourth part of

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weakened by no operation, but what the nitrous acid itſelf has undergone." BEWLEY'S TREATISE ON AIR, p. 101.

† Dr. Prieſtley ſays,—"I expoſed to this nitrous vapour, *common air*, *inflammable air*, and *fixed air*, and all of them for a conſiderable time, without making the leaſt ſenſible alteration in any of them. It is poſſible that a longer continuance of the proceſs might have affected them; but a great deal leſs time was abundantly ſufficient for this acid vapour to produce its utmoſt effect upon nitrous air."—Vol. II. p. 171.

the nitrous air remains. Beſides, it is making the nitrous air decompound the nitrous acid; or *vice verſa*, the nitrous acid the nitrous air. But according to my theory, the nitrous acid applied in the powerful ſtate of vapour, (for water alone can calcine iron, when aided with heat in the form of vapour) will penetrate into the nitrous air, attacking its phlogiſton, and producing an effervеſcence, ſo as to decompound the nitrous air.

In attending to the different writers upon phlogiſton, either for or againſt it, the following reflection neceſſarily occurs; that of the phenomena of the numerous experiments which have been made, ſome are made uſe of to contradict the one hypotheſis, and ſome the other. However, it appears from their own writings, that the great body of the phenomena contradict both hypotheſes. As Dr. Higgins has treated extenſively of this ſubject in his late publication, I beg leave to make a few obſervations upon it.

Dr. Higgins ſays, p. 200,—"Equal parts of "dephlogiſticated and light inflammable air, "mixed over water, will form, according to Mr. "Kirwan, a denſe white cloud; more than one "half is abſorbed, and is found to be common "marine acid, and the reſiduary air is pure in-"flammable air."

Now, this fact is certainly a very ſtrong proof that there is ſuch a body as phlogiſton, and that it forms dephlogiſticated marine acid into the common marine acid. Dr. Higgins, to combat this ſtrong experiment, ſays,—"That there is no "fixed air formed in the proceſs," which, according to Mr. Kirwan's theory, there ought to be. *So one error is oppoſed to another.* There certainly is not any fixed air formed, nor ought there to be any formed in the proceſs. Our chemiſts of *great name* are ſo enveloped in, and embarraſſed with theories, that they find themſelves aground in expounding any phenomenon.

Dr. Bewley, ſays, p. 173,—"There is an expe-"riment mentioned by Dr. Higgins, which he "ſays in favour of Mr. Lavoiſier's ſyſtem; as "indeed we muſt agree with him, that it is "directly contradictory to the ſyſtem of phlogiſ-"ton, with the belief that dephlogiſticated air is, "in reality, dephlogiſticated. But if Dr. Higgins "and others will pleaſe to ſee the truth, this de-"phlogiſticated nitrous air is formed of the ni-"trous acid and the phlogiſton of the copper:— "it will be a ſtrong proof in favour of the doc-"trine of phlogiſton. The Dr. ſays, p. 550,— "If tin be introduced into a neutral ſolution of "the tin in the nitrous acid, it is calcined, a calx "is thrown down, and the dephlogiſticated or "imperfect nitrous air is produced. Dephlogiſ-"ticated nitrous air, according to the phlogiſtians, "contains no phlogiſton; then I aſk, what becomes of the phlogiſton of the newly calcined "metal? If tin contained phlogiſton, either inflammable air or nitrous air would be produced, "or a portion of the diſſolved tin would be precipitated in its metallic ſtate; neither of which "will take place, if the experiment be well conducted. Hence I ſhould ſuppoſe, that metals "do not precipitate each other in their metallic "ſtate, in conſequence of a double affinity proceeding from the matter of light inflammable "air, or phlogiſton; and likewiſe that metals "part with no ſuch thing during their calcination "in acids."

Dr. Higgins ſays, in his laſt publication, p. 49, —"I introduced ſome iron nails, free from ruſt, "into ſtrong volatile vitriolic acid; when it ſtood "for a few minutes, it acquired a milky appearance, and the ſolution went on without ebullition or extrication of air. On ſtanding for a "few hours, the ſolution acquired a darkiſh "colour, and a black powder was precipitated. "This powder, when collected and waſhed, put "on red-hot iron, burned partly like ſulphur, and "and partly like charcoal duſt, and the incombuſtible reſiduum was of a purpliſh colour. "The filtered ſolution was perfectly neutralized, "and free from the leaſt ſulphurous pungency.— "Its taſte was ſtrongly chalybeate, but not ſo

" diſagreeable as that of the ſolution of iron in the " perfect vitriolic acid, or in any of the mineral " acids.

" Nitrous acid dropped into the ſolution in- " ſtantly produced a cloudineſs, which immedi- " ately diſappeared without ebullition, though " volatile ſulphurous acid was diſengaged in its " utmoſt degree of pungency. The vitriolic " marine, and nitrous acids decompoſed the ſolu- " tion, but cauſed no turbidneſs, nor was any " inflammable air produced."

Nothing can be more obvious than this experiment. The volatile vitriolic acid being previouſly united to a large proportion of phlogiſton, conſeqnently united itſelf to the phlogiſton of the iron in a very gentle manner; its activity being taken away by the previous ſaturation. However it decompounds the iron, attracting a quantity of its phlogiſton ſufficient to form ſulphur. This agrees with my theory; that when the acid and the phlogiſton unite ſo gently as not to produce any great heat, they form no kind of air; neither the vitriolic acid, nor inflammable airs, but ſulphur. Of which this is a proof; if you drop into the ſolution another acid, which has a ſtronger attraction for the metal's phlogiſton, the volatile vitriolic acid will be ſet looſe in the ſame ſtate in which it was applied to the iron.

The preſent chemical doctrines have given riſe to very ſingular theories and conſtructions of the different phenomena. Dr. Higgins ſays, p. 163, —" From the proceſs of theſe experiments, I did " not heſitate to conclude, but that which is called " dephlogiſticated nitrous air, is common nitrous " air deprived only of a portion of its dephlogiſti- " cated air." The experiment which gave birth to theſe opinions is this; by expoſing nitrous air to iron, the iron is reduced into a calx, and the nitrous air turned into a dephlogiſticated air.*

Let us try in what manner this coroborates my theory. The acid which is not neutralized in nitrous air, (this is ſeen by its turning the vegetable juices red) will act upon the phlogiſton of iron and unite with it, receiving ſo ſtrong a ſaturation as to form this air, in which a candle burns with an enlarged flame. From the experiment which Dr. Higgins made, he is ſo ſtrongly confirmed that, dephlogiſticated air is imbibed by the calx of iron, as to ſay,——" Theſe facts leave " no room to doubt, but that dephlogiſticated air " contains leſs dephlogiſticated air than the com- " mon nitrous air." What muſt we ſay of doctrines which lead thoſe who maintain them to ſuch concluſions? That an air which admits a candle to burn in it with ſuch a ſtrong and enlarged flame, ſhould poſſeſs leſs dephlogiſticated air, than nitrous air which immediately extinguiſhes flame, the ſame as pure phlogiſticated air! Such however are the errors which flow from theſe doctrines.

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* But this air is very different from the oxygen gas of chemiſts, or pure air; for it is as noxious to animal life as the moſt mephitic air.

The above mentioned author ſays, (laſt publication, p. 164)—" Dr. Prieſtley has diſcovered a " ſpecies of nitrous air which ſupports combuſtion, " deſtroys animal liſe, and is condenſible in water. " This he has called dephlogiſticated nitrous air. " I conſider dephlogiſticated nitrous air to be the " laſt ſtage of nitrous acid, and to be leſs underſtood than the four preceding. I expoſed four " equal quantities of nitrous air in different tubes, " to a nearly equal proportion of iron and water. " In three weeks the air was diminiſhed one ſixth, " and the reſiduum extinguiſhed a candle, and " reduced common air. In three weeks more it " was reduced about one fourth, and the reſiduum " ſuffered a candle to burn in it faintly. When it " ſtood a fortnight longer, the diminution was " nearly one third of its bulk, and a candle burned " in the reſiduum with an enlarged flame. I let the " other tube ſtand until the air contracted to more " than one third of its original bulk; the reſiduum " was phlogiſticated air and had the ſmell of volatile alkali.* From the progreſs of theſe expe-

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* This alſo agrees with my theory. I ſuppoſe that the volatile alkali is formed of a neutralization of fire, ſimilar to inflammable

"riments I did not heſitate to conclude, but that "which is called dephlogiſticated nitrous air, is "common nitrous air, deprived only of a portion "of its dephlogiſticated air. Dr. Prieſtley found "that nitrous air, which ſtood in contact with iron "and water for four months, extinguiſhed a can"dle. He likewiſe found that a candle burned "with an enlarged flame in nitrous air which had "been in contact with iron, over mercury, about "ſix months. The ſame philoſopher found, that "nitrous air, expoſed to liver of ſulphur for a day, "was diminſhed one third of its bulk; a candle "burned in the remainder with an enlarged flame, "and it was not in the leaſt diminiſhed by nitrous "air. I have frequently obſerved that nitrous "air, when reduced to two thirds of its bulk, "always admitted a candle to burn in it with an "enlarged flame; but that in proportion as it "got below this ſtandard, it ſupported flame ſo "much the worſe, until it was reduced nearly to "one third, when it extinguiſhed a candle."

I have quoted this paſſage, becauſe the phenomena mentioned in it, Dr. Higgins ſays, are not preſumed to be accounted for by their theory. I beg leave to mention my explanations to this learned ſociety. Airs in which bodies burn are

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inflammable air; and therefore in the laſt ſtage of this proceſs it attracted the inflammable air, or phlogiſton of the air, and left it in a ſtate of azote, which I ſuppoſe the leaſt ſaturation of phlogiſton.

combuſtible as well as the bodies themſelves. They conſiſt of an acid and concentrated fire; and the reaſon why they are neceſſary to combuſtion is, that their fire is more eaſily ſet looſe than that of the combuſtible bodies; in conſequence of which, the ignition of the airs produces the ignition of the combuſtible bodies. An author already quoted ſays, (Treatiſe on Air, p. 29.) "A combuſtible "body, though fire is applied to it will not burn "without the agency of another body. The great "agent in nature is atmoſpherical air, which is a "compound of an acid and phlogiſton. But being "compoſed of a mild acid, and not poſſeſſing "a high concentration of fixed fire, parts with its "fixed fire, when actual fire is applied to it; "however, the heat it produces is not powerful "enough to keep the air burning, or decom-"pounding its own fixed fire, unleſs it is expoſed "to ſome combuſtible body having a high concentration of fire."

The reaſon why Homberg's pyrophorus will burn in nitrous air, but not in dephlogiſticated air, is (as I have proved in my thoughts on air) that all pyrophori conſiſt in being formed of a looſely attracted fire, like lime; conſequently acids and acid airs will expel it. And, for this reaſon, if we drop the nitrous acid upon the pyrophorus, it will turn it into a red hot coal. That dephlogiſticated nitrous air will not decompound pure air, is owing to its acid being perfectly neutralized with phlogiſton, and for the ſame reaſon it will not turn the vegetable juices red, which nitrous air will do.*

As chemical inveſtigations are at preſent the favourite purſuit, and as experiments are multiplied, ſo each chemiſt being under the influence of ſome theory, endeavours to find out experiments which may favour it. The fellowing aſſertions may, perhaps, be thought too bold; yet I will venture to ſay, I never heard of one chemical experiment but may be rationally explained upon the principles of my theory, and upon no other.

I am happy to find that Dr. Prieſtley, after diverſifying his experiments upon the burning of inflammable and pure airs, has at laſt drawn the ſame concluſions I did ſome years ago. In my thoughts on air (publiſhed in 1785) is the following paſſage, (p. 328.) "We cannot poſſibly miſ-
"take theſe experiments; when the inflammable
"air was in leſs proportion to the empyreal air,
"the reſiduum of water, after the exploſion, was
"acid; but with a greater proportion of inflam-
"mable air the reſiduum of water had not this

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* I mention theſe experiments, as being inexplicable (agreeably to Dr. Higgins) by their theories. And from the nitrous acid ſetting looſe ſo inſtantaneouſly the fire of the pyrophori, ſhews clearly in what manner it operates upon volatile oils, ſetting looſe their fixed fire.

“ acidity. And likewiſe this acid reſiduum was
“ of the nitrous kind. As then the acidity of the
“ water and phlogiſtication of the air alternate
“ with each other, as the one takes place the other
“ is not to be found: we cannot have a doubt but
“ it is from this cauſe. That when there is a ſmall
“ proportion of inflammable air exploded with
“ the empyreal air, the fire will be ſufficient to
“ ſet looſe the quieſcent fire or phlogiſton of the
“ empyreal air, ſo as to turn it again to the ni-
“ trous acid from which it was made; but when
“ there is a greater proportion of inflammable
“ air, ſo as to give ſufficient phlogiſton to the
“ acid to unite with it, they forcibly attract each
“ other, and generate phlogiſticated air which
“ makes the exploſion. But this phlogiſticated
“ air is undoubtedly made by the union of the
“ acid ſet free from the decompounded empyreal
“ air, and united with the phlogiſton of the in-
“ flammable air: for the pureſt empyreal air is
“ made uſe of in the proceſs. And this ſame em-
“ pyreal air may, by burning or by bees breathing
“ in it, be all turned to an acid air; therefore there
“ cannot be the moſt diſtant ſuppoſition of this
“ phlogiſticated air being previouſly formed in the
“ empyreal or inflammable air; but is in reality
“ generated at the time of the exploſion.”

And in my letter to Dr. Prieſtley and others, (publiſhed in 1788,) I thus expreſs myſelf, (p.

125.) "Or take a quantity of the empyreal air, "and burn it with a ſmall proportion of inflammable air, and there will be an acid in the reſiduum; and then take the ſame quantity of the "ſame kind of empyreal air, and burn it with a "full proportion of inflammable air, and there "will be no acid left." Again, (p. 127.) "This "theory is moſt ſtrikingly ſhewn, in firing inflammable air and empyreal air. If there is a "full proportion of inflammable air, there will be "ſuch a degree of fire ſet looſe, as to fly off with "the acid; (as we have before explained,) but if "there is a leſs proportion of inflammable air, ſo as "not to produce ſo great a heat, the acid will be "left in the reſiduum."

According to their theories it was ſuppoſed, that in theſe experiments a ſurplus of either air would remain unchanged after the operation, without effecting the reſult. This ſhould really be the caſe, were theſe two airs united together upon the ſame principle as that of two bodies ſaturating each other. But as it is a proceſs of burning, conſuming, or ſetting free fixed fire, which makes ſo conſiderable a part of theſe airs, ſo the explanation now given of it by Dr. Prieſtley, is in direct oppoſition to their theories. He ſays there is an acid in the reſiduum, and phlogiſticated air is a compound body, formed of the nitrous acid and phlogiſton. How then can phlogiſticated air be one of the bodies which goes to the compoſition of the nitrous acid? This experiment, if due attention is paid to it, can be explained only on the principles of my theory.

I flatter myſelf that this very learned ſociety will, without prejudice or prepoſſeſſion of any kind, candidly conſider and weigh my explanations of the different chemical phenomena mentioned in this paper. For I am apt to believe, that the theories which modern chemiſts have been, from their experiments, led to adopt, will, upon diſcuſſion, be found inadequate to account for theſe phenomena; but that mine gives a conſiſtent and ſatisfactory explanation of them all.

Chemical philoſophers have been imperceptibly lead to adopt their preſent hypotheſes without conſidering the wild concluſions deducible from them. They have ſuppoſed moſt bodies in nature, to be formed of airs; the waters of the ocean, acids, ſalts, &c. But it is an incontrovertible fact, that in making any body, the ingredients of which it is compoſed, muſt be previouſly formed before they are united or mixed. Then, I would aſk philoſophers, what ſpace that we are acquainted with, would be ſufficient to hold theſe ingredients; viz. pure, inflammable, phlogiſticated, and nitrous airs? The great expanſive ſpace between planet and planet would be inſufficient. Here nature would be inadequate to herſelf.—Again; if water is formed of pure and inflammable airs; and (conformable to the preſent theories) with ſuch an immenſe quantity of caloric, as we have ſhewn it muſt poſſeſs, what would follow from this? For inſtance, in the ſolution of iron, they ſay water is decompounded, one of the bodies forming inflammable air, and the other very pure air which enters the calx. The *aurum fulminans* is capable of exploding with great violence. All this fire (agreeably to the doctrines of modern chemiſts) comes from the condenſed pure air in the calx, uniting with the inflammable air of the volatile alkali. But were this ſame condenſed pure air to explode with the inflammable air in its aerial ſtate, it muſt produce a far more wonderful exploſion, as it would have the amazing quantity of caloric the inflammable air poſſeſſes in its aerial ſtate. Dr. Crawford, from his experiments, ſays, the caloric it poſſeſſes is 21,4000; whilſt dephlogiſticated air poſſeſſes only 4,7490. From this it would appear, that water poſſeſſes more caloric than the two bodies of which it is formed, in their aerial ſtate. Therefore, in burning theſe two bodies into water, how can we agree with Mr. Lavoiſier, who ſays they give out caloric in this quantity:

From 1 lib. of hydragon gas 295,58950.

From 1 lib. of oxygen gas 52,16280.

They ought, in their formation into water, to have produced ſo much cold.

Gentlemen, ſurely theories which lead to concluſions ſo very extravagant, ſhould be more maturely conſidered before chemiſts adopt them. Were this the caſe, they would be diſcovered to be founded in error. The ocean is formed of the two moſt combuſtible bodies in nature; but at this rate, preſumptuous man, might have it in his power, with ſuch an immenſe quantity of caloric, as we are told the water poſſeſſes, to ſet it on fire! But the Deity takes ſuch care of his works, that man, though willing, cannot have it in his power to deſtroy any of them. He may make experiments, adopt theories, and draw wrong concluſions from them; but he ſhall not be able to burn the planetary ſyſtem, nor even ſet this little world on fire.

## OBSERVATIONS UPON DR. FORDYCE'S PAPER IN THE PHILOSOPHICAL TRANSACTIONS.

THE chemical philoſophers have, for a long time, been labouring under the very extraordinary idea, that water is compounded of inflammable and pure airs. This muſt have originated from their miſconſtruing their own experiments. An idea, which, when maturely conſidered, bears evident marks of abſurdity. For, in purſuing this opinion, the Royal Society is brought to believe, that the water of the river Thames has been ſet on fire,—

nay, that it thunders in its combuſtion.* That water ſhould be depoſited, when inflammable, and pure airs are fired, is perfectly agreeable to my ſyſtem; which ſuppoſes that all airs have water for their baſis, water being ſimilar (as I have before ſhewn) to the water of compoſition and chryſtalization in the neutral ſalts. Dr. Fordyce, who for accuracy and ability as a chemiſt, deſerves the greateſt attention, has given to the Society a paper, proving that the additional weight gained by the calx of zinc, above its metallic ſtate, is certainly given to it by the water employed in the proceſs. He ſays, in the volume for 1792, p. 382,—"And therefore the "matter occaſioning the additional weight of the "calx, above that of the metal and the inflam-"mable air, are both produced from the water." In which concluſion we perfectly agree; but the moſt important queſtion introduces itſelf, whether this additional weight is pure water, or a decompoſition of the water? Moſt bodies are compounded; and to decompoſe them, you muſt do it by chemical attraction. The zinc made uſe of by Dr. Fordyce is (I ſuppoſe) formed of fixed fire, and an earth; by adding the vitrolic acid and water to the zinc, they decompound it, attracting its fixed fire, and the earth, the zinc requiring their joint influence. It is juſt the ſame with lime fixed air and water. Lime has gotten a ſaturation of fixed fire, which the fixed air and water cannot ſet looſe by themſelves; but, by their joint powers, they can perform it, uniting with the calcareous earth, and expelling the fire.†

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* See Dr. Pearſon's paper upon the decompoſition of fixed air:

The calx of the zinc, in the Dr.'s experiments, was united to the vitriolic acid and water; and, upon adding an alkaline ſalt to the ſolution, it united to the acid, from its ſuperior attraction for it. But then the calx looſing one of its component bodies, would ſaturate itſelf with more of the water. Nay, we find that water alone, in the powerful ſtate of ſteam, will form metals into calces; iron, for inſtance.

My ſyſtem ſuppoſes, that the gravity of inflammable air is principally from water, with a little of the vitriolic acid and the earth of the metal, neceſſary to chemically attract and fix the fire.—Agreeably to Dr. Fordyce's accurate experiments, in nine grains of inflammable air, there would be two grains of the acid; as that weight of the acid diſappeared: which is full as great a proportion as I thought it might poſſeſs.

Now, as the reſult perfectly agrees with both hypotheſes, (viz. the one generally received and mine) the only way to elucidate which is the true one, is to endeavour to aſcertain, whether this additional weight of the calx is from pure air or water. Inflammable air will precipitate the body which the calx is united to, forming water; but then, they ſay, the water is formed from the union of the inflammable air and the pure air of the calx, which they ſuppoſe it to poſſeſs. I took ſtrong concentrated nitrous acid, (what they call dephlogiſticated acid) as free from water as poſſible, and added it to the calx of zinc, calcined and formed the ſame, as in the Dr.'s experiments. As the earth of the calx has a ſtronger attraction for the acids than for the water, it would precipitate part of the water, if it poſſeſſed any. Upon adding the nitrous acid (but any of the mineral acids will equally do the ſame) in ſuch proportion, as to ſaturate the calx, there appeared a ſenſible moiſture upon its ſurface; and to aſcertain what that moiſture conſiſted of, I expoſed to the calx a ſtrong heat, and I got a conſiderable quantity of water from it. The calx was expoſed to a ſimilar heat, before I added the acid, and no water came from it. It retained its water by chemical attraction, and therefore the fire was not capable of expelling it. That water of itſelf will form metals into calces, by expelling their fixed fire, is not to be wondered at; for it will expel the fixed fire from the acids. When applied to ſulphur, in the form of ſteam, it will unite with its fixed fire, producing inflammable air, as Dr. Prieſtley found.

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† See a full elucidation of this doctrine in a paper of mine in the Gentleman's Magazine for 1792.

That the moiſture which came from the calx, upon its being united to the concentrated mineral acids, did not come from the acids, I aſcertained by adding the ſame acids to earths, which poſſeſſed no water; lime for inſtance, and I got no water from them.

But to diverſify the experiments of Dr. Fordyce: inſtead of adding the cauſtic alkali to the vitriolic ſolution of the zinc, let him precipitate the calx with the mild alkali; and the calx will be precipitated with part of the fixed air of the alkali. Now, if the calces of metals conſiſt of the earth and pure air, by the addition of the fixed air to the calx of zinc in this experiment, it will weigh ſo much heavier, according to the quantity of fixed air it got from the alkali; which is very wonderful. But I found the calx which was precipitated by the cauſtic alkali, (formed from an equal quantity of the metal) only twelve grains lighter, as that which was precipitated by the mild, though it ought to have been twenty grains lighter, from the quantity of fixed air which diſappeared in the experiment.

But an experiment which I made ſome time ago, before I read Dr. Fordyce's experiments, is a proof of this; viz. I took a dram of mercury, and calcined it in the nitrous acid, taking care to aſcertain the exact quantity, and adding as much of the acid as was neceſſary to its calcination; I likewiſe carefully collected the nitrous air produced in the proceſs. I then ground the calx with ſpirits of ſal ammoniac, the nitrous acid left the calx, and united itſelf to the volatile alkali, forming the ammonical nitre; and accurately weighed the quantity it yielded. After this I took the ſame quantity of the nitrous acid, as I uſed in the ſolution of the mercury, and neutralized it with the ſpirits of ſal ammoniac, and I got only about five grains more of ammoniacal nitre, than I got when the acid was ground with the calx. The loſs of the acid I attributed to that which went to the formation of the nitrous air.

But, agreeably to Mr. Lavoiſier's hypotheſis, all the nitrous acid ſhould have been decompounded into nitrous air and pure air; for, as I added no more acid than was neceſſary to ſaturate the mercury, ſo there ſhould have been no acid left in the proceſs: but the whole, appeared except what the nitrous air took up, which was rather more than the inflammable in Dr. Fordyce's experiment of the vitriolic acid; his being only two parts out of nine:—mine was in the proportion of three parts out of nine. But ſtill to aſcertain the reſult with greater accuracy, I revived this calx of mercury, without addition, merely by the influence of the fire, and carefully attended to what came from it upon its reduction, and I found nothing but pure water. But, if the operator is not careful in receiving all the acid from the calx, there will be a little oxygen gas produced. That there might no ſuſpicion riſe, that the calx got any inflammable air from the ſpirits of ſal ammoniac, I uſed in the proceſs the cauſtic fixed alkali, or the *kali purum*, and there was directly the ſame reſult; nothing but water appearing at the reduction of the mercurial calx.

I have repeated Dr. Fordyce's experiment, uſing the nitrous acid inſtead of the vitriolic, and iron inſtead of zinc, as zinc, with the nitrous acid, forms an imperfect nitrous air; and the reſult was perfectly agreeable to my hypotheſis: the nitrous acid was no more decompounded than the vitriolic, both acids being found entire: therefore theſe obviouſly plain experiments ſufficiently refute the opinion of the acids being compounded bodies. But that my experiments may not be implicitly received, I refer the repetition of them to the accurate Dr. Fordyce: he has been bred in the old ſchool, and I hope will not too haſtily receive the ſingular and hoſtile (I had almoſt ſaid monſtrous) opinions, which are at preſent adopted by many.

## OBSERVATIONS UPON DR. PEARSON'S PAPER IN THE PHILOSOPHICAL TRANSACTIONS.

DR. Pearſon's paper upon the decompoſition of fixed air, comes next under our conſideration.— The variety of experiments made by the numbers now employed in chemical purſuits, have given us a variety of phenomena; and the phenomena have been forced in, head and ſhoulders, to account for the very ſingular hypotheſes now adopted. But I will make uſe of this bold aſſertion, that there is not one of their experiments, but finds an eaſy and rational explanation upon the principles of my ſyſtem; while their explanations are in direct oppoſition to the phenomena. We ſhall find this obſervation to be verified in theſe experiments of Dr. Fordyce and Dr. Pearſon.

The Dr. after giving us a hiſtory of affinities, which are a maſs of ſtrange contradictons, endeavours to take from Mr. Tenant the merit of being the firſt to decompound fixed air; ſaying that the chemical affinities are in oppoſition to his experiments, imputing the charcoal formed to the phoſphorus; while the direct ſame charge might be made to his. Thoſe who repeat the Dr.'s experiments, will find very different reſults. But the obſervations I have made from expoſing the fixed alkalies and phoſphorus to heat, are, that if the alkalies retain their water, the water acting upon the phoſphorus will expel its fixed fire in the ſtate of inflammable air. But if they are ſo far bereft of their water, that there is not ſufficient to form the fixed fire into inflammable air, (which I have proved is neceſſary to the aerial forms) it will take the form of a kind of ſoot, the ſame as other phlogiſtic bodies will do, when expoſed to fire; viz. coals, &c. when not actually ignited: which ſoot will unite itſelf to the materials employed in the operation; and which Dr. Pearſon ſeparated from them; for it, according to the quality of ſoot, will not mix with water.

Phoſphorus, when burned in the open air, has ſuch a tendency to ignition, that it will take fire of itſelf, and imbibe the water and fixed air, of which the atmoſphere is formed. This ſhews that the phoſphoric acid has a ſtrong attraction for both theſe bodies. Therefore, theſe two bodies operating upon the phoſphorus, under an intenſe degree of heat, while the alkali has a ſtrong attraction for its acid, will produce a decompoſition; the water and fixed air entering the phoſphoric acid, and expelling its fixed fire. The obſervations I have made upon theſe experiments are, that if you deprive the alkali of too much of its water, it will not decompound the phoſphorus ſo readily, the water aiding the fixed air in the decompoſition. And if you leave the alkali with all its water, it will the more readily decompound the phoſphorus; but then it is in that abundance, as to give an aerial form to the fixed fire, in the ſtate of inflammable air.

That phlogiſtic bodies may be decompounded by water alone, we have many inſtances; ſteam paſſing through ſulphur, iron, charcoal, &c.—But,

as I elſewhere obſerved,* even in bodies which contain not an atom of fixed air, the phoſphorus will expel part of its fixed fire, when expoſed to heat, in the ſame manner as when it is united to lime. Nay, it appears by a gentleman in the correſpondence of the Monthly Review, even when treated by itſelf, it will depoſite this ſooty matter upon its paſſage through glaſs.

We ſhould not be ſurpriſed, if phoſphorus is decompounded under this intenſe degree of heat, and ſurrounded by ſuch an active body as an alkali. "If ſulphur be digeſted in oil of turpentine, and then ſlowly diſtilled for ten or twelve days, it will be converted into vitriolic acid, according to Homberg, M. P. 1703." Then need we wonder at phoſphorus, which is a far more combuſtible body than ſulphur, being decompounded, looſing its fixed fire in theſe experiments?

The black ſooty matter, which they call charcoal, is ſeen in other experiments, where there is an imperfect combuſtion, viz. in firing inflammable air got from metals and pure airs, there is often (as Dr. Prieſtley found) a depoſit of this colouring matter, a part of the fixed fire of the inflammable air being not ſet free. But I ſhould ſuppoſe they will not argue that this metallic inflammable air poſſeſſes any charcoal; but its appearance is accidental, depending upon the perfect or imperfect combuſtion. That it is not charcoal, is clear; indeed the giving it a ſerious diſcuſſion appears to me ridiculous. If any chemiſt will ſeriouſly conſider the teſts that they put it too, it will appear plainly that it is this ſooty matter I ſpeak of.

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* See the Gentleman's Magazine, 1792.

To ſhew how warmly a favourite hypotheſis will be ſupported, Dr. Pearſon acknowledges he got the ſame ſoot from the cauſtic alkali; but then he has this ſalvo, that he could obtain no alkali but what poſſeſſed ſome fixed air. But if he will look ſharp, he may procure ſome that poſſeſſes not an atom of fixed air; nor will produce any when added to acids; which is a *certain teſt*. And which cauſtic alkali will, with phoſphorus, produce this ſooty matter; but, by adding a very little water to the alkali, the leaſt being ſufficient, ſo as to aſſiſt in the decompoſition of the phoſphorus, though not enough to give its fixed fire an aerial form, but only this ſooty one: they will together produce it.

The Dr. ſays,—" I am very fully aware that the proportion of reſpirable air and charcoal produced in this experiment, do not correſpond to the proportions of them, we ſhould have expected conſiſtently with the ſynthetical experiments concerning carbonic acid. The variation is eſpecially great with reſpect to reſpirable air, of which there ſhould have been *eighteen* grains inſtead of *five*, to combine with the whole of the charcoal." Indeed, his experiments and deductions are ſuch a confuſed maſs of inconſiſtencies and falſe concluſions, that Sir Joſeph Banks, I ſhould ſuppoſe, cannot clear himſelf to a learned and candid world, from the charge of partiality, in receiving his paper, and rejecting mine. Even theſe five grains of pure air, which the Dr. ſpeaks of, is only ſuppoſed from their theory, as the phoſphoric acid is formed in a ſmall degree, and it is ſuppoſed to be formed from pure air.

But the following is an experiment ſo obviouſly clear, that it is impoſſible to miſtake it. Take pure diſtilled water, and impregnate it with as much fixed air as it will take up in ſolution, marking the quantity abſorbed, then expoſe them to the rays of the ſun for ſome time, after which expel the air from the water by fire, and you will find a quantity of pure air formed. That it is the fixed air which formed the pure air, is certain, as a great proportion of it diſappears in the experiment.

Dr. Prieſtley ſays, vol. II. p. 219, "In order to "be more ſure of this fact, I was more eſpecially "careful, the ſecond time that I made the experi- "ment, to uſe every precaution that I could "think of, in order to prevent any error in the "concluſion. For this purpoſe, I took rain- "water, and boiled it about two hours, in order

" to get it perfectly free from air; and I began to " impregnate it with fixed air a long time before " it was cold, and therefore before it could have " imbibed any common air; and, in order to ex- " pel the air from it, I put it into a phial, which " I plunged in a veſſel of water ſet on the fire to " boil, taking care that both the phial containing " the impregnated water, and the glaſs-tube thro' " which the air was to be tranſmitted, were com- " pletely filled with the water, and no viſible par- " ticle of common air lodged on the ſurface of " it. I alſo received the expelled air in water, " which contained very little air of any kind, leſt " the very ſmall degree of agitation which I made " uſe of, in order to make the water re-imbibe the " air, ſhould diſengage any air from it. Alſo, " that leſs agitation, and leſs time, might be ſuf- " ficient, I chiefly made uſe of lime-water for this " purpoſe. But notwithſtanding all theſe precau- " tions, I found a very conſiderable reſiduum of " air, not leſs than Mr. Cavendiſh had ſtated, that " water would not imbibe."

Now, can either of theſe experiments be reconciled to the new theory? Yet they are ſimple and plain; no crucibles or gun-barrels to look through. Upon the fixed air forming pure air, a quantity of charcoal ought to have appeared; and as the black colouring matter is the great characteriſtic of charcoal (agreeably to the decompounders of fixed air) we ſhould have ſeen the moſt minute tinge in the pellucid water: but it continues through the operation perfectly pellucid and tranſparent.

Now, this ſimple experiment of Dr. Prieſtley's is *poſitively only to be accounted for by my ſyſtem, as being in direct oppoſition to all their opinions.* Here is a quantity of fixed air turned into phlogiſticated air, clearly and obviouſly, beyond the moſt vague conjecture, to ſuppoſe to the contrary. There was nothing but fire and water that were employed in the proceſs to produce the change. It cannot poſſibly bear any other explanation than this, that the fire ſaturated itſelf with the fixed air and water, ſo as to take off the aerial acid's acidity; but not to give it an impregnation or ſaturation of fire ſufficient to form pure air, which the fire of the ſun, operating by ſlow degrees, gives it, producing pure air. Their preſent hypotheſis ſuppoſes phlogiſticated air an element, and that fixed air and water are not formed of it.* From whence does it come then? Here one body diſappears, and another appears, agreeably to the proper proportions. Will no admonition of mine call forth that candour and reaſon that ought to take place in philoſophical diſquiſitions? I call upon an explanation of theſe two ſimple experiments. *What muſt the candid world think?*

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* Dr. Prieſtley, from his late experiments. thinks that phlogiſticated air is formed from an acid and phlogiſton. Here, obviouſly to the ſenſes, fire produces the ſame effect.

Mr. Lavoiſier, by an experiment, attempts to prove, that charcoal is one of the conſtituent parts of fixed air; which is this, by burning charcoal in pure air, a very ſmall reſiduum of the charcoal is left; while there is a large quantity of fixed air formed, nearly equal to the weight of both the pure air and the charcoal. But if we will conſider what pure air is formed of; of the aerial acid and water, neutralized by fire; and charcoal of the vegetable acid, water, and fire,† part of which fire is ſo united to the acid that, upon cumbuſtion, it forms the aerial acid, commonly called fixed air. That in forming wood into charcoal, the heavy earthy parts go off united to the heavy inflammable air. Dr. Prieſtley even found the heavy earths of metals intermixed with nitrous airs and inflammable airs; and that, upon ſtanding over water, they would depoſite a part of their earth.* And he likewiſe

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† The immortal Scheele ſays, p. 182,—"The charcoal is "moſt proper for the purpoſe, ſince it is a ſulphur compounded "of phlogiſton and aerial acid. If coals be ground together "with alkali, made cauſtic by quick-lime or fire, and then diſ-"tilled in a glaſs retort in an open fire, a great quantity of in-"flammable air is thus obtained, containing no aerial acid: if a "bladder be tied to the mouth, the alkali on the other hand "loſes it cauſticity, and effervеſces with acids."

* Magneſia, by calcination, looſes part of its earth. See Bergman's Eſſays, vol. I. p. 42.

found

found that the nitrous acid mixed with ſpirits of wine, &c. and aerilized, would be formed into the aerial acid; need we wonder then at the quantity of the aerial acid being found in the burning of charcoal along with pure air, formed from nitre, &c.

Can we have a more convincing experiment of what charcoal is formed of, viz. in paſſing the ſteam of water through charcoal, which forms two kinds of airs, fixed and inflammable airs. The vegetable acid of the wood, by being charred, having got a ſaturation of fire, in one caſe to form inflammable air, and in the other fixed air; the latter being a ſaturation juſt ſufficient to give the acid a permanent aerial form. The ſteam giving to both the airs the water for their aerial compoſition.—This is the true explanation, without running into the wild ſuppoſition of water being formed of both theſe airs.

Even acids are ſeparated from alkaline ſalts, by the influence of actual fire; and every chemiſt muſt allow, that there is a ſtrong attraction between thoſe bodies; therefore we need not be ſo wonderfully ſurpriſed, if phoſphorus can be ſo decompounded. That neither it nor ſulphur can be decompounded by heat alone, without the aid of other bodies, is moſt probably owing to the volatility of phlogiſton. This is exemplified in alkalies. The fixed alkali can be bereft of its aerial acid by fire; but the volatile alkali cannot. But by lime, the volatile alkali is as ſoon made cauſtic as the fixed alkali. That, by the powerful influence of fire, both ſalts are ſometimes aerilized with acids, appears from nitre being formed into air, from *nitrum flammans* the ſame.

In forming the calcareous earths into lime, in attending to the proceſs accurately, we may obſerve different phenomena; at firſt the fixed air and water are expelled, and the earth ſaturated with looſe fire. But if you puſh the proceſs farther, the fire will, inſtead of taking this looſe ſaturation, become more fixed, penetrating the earth, ſo as to become alkaleſcent: and in this ſtate, the lime becomes light and ſpungy, having loſt part of its earth, and will not do for mortar, the workmen rejecting it, as being uſeleſs; for it will not ſplit and fall into powder with water.—I have, by carrying on the proceſs for a long time, made it almoſt an alkaline ſalt, turning it perfectly mild, in reſpect to its cauſticity, and having the properties of alkalies in moſt of its qualities.*

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* I have proved, in my Thoughts on Air, that the cauſticity of lime does not depend upon the abſence of its fixed air, but upon its ſaturation of looſe fire; which fire, when it becomes active by water, &c. burns or conſumes bodies. Now, this experiment proves it: for, when the fire is ſo puſhed in the proceſs, as to be more fixed in the earth, ſo that moiſture, &c. cannot expel it, the lime then becomes mild;—and it will bear

Dr. Pearſon's paper mentions a rediſh brown powder, which he obtained from lime and phoſphorus; and which, upon being thrown into water, produces a vapour that fulminates with the atmoſphere. This fulminating vapour, agreeably to the preſent hypotheſis, decompoſes the water. Need I mention that the fire, which the phoſphorus and lime receive, and being ſo exſiccated, that, upon coming in contact with the water, they imbibe or attract it; and ſo much fire is ſet looſe from the lime, &c. which, along with the water, aerilizes the phoſphorus; and probably may ſet a part of its fire looſe, as inflammable air, as there is plenty of water to give it an aerial form: and not the very ſingular and extraordinary idea, that the water is decompounded; and, upon its ignition, it *thunders*. How long will our modern chemiſts go on with their experiments to ſupport ſuch hypotheſes?— Human knowledge is weak; and in the inveſtigation of abſtruſe ſubjects, very weak opinions have been received. This we ſee often exemplified. But then, when a regular ſyſtem has been offered, which embraces all the phenomena; for I muſt again repeat, that there is not one experiment of the

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no other explanation. The idea of its wanting its acid to neutralize the earth, is quite inadequate to the phenomena; for it ought in this caſe to have become more cauſtic at the latter part of the proceſs, inſtead of its becoming perfectly mild, and of an alkaleſcent quality.

the many thouſands that have been made, which my doctrine does not fully and rationally account for. Then what muſt we think of our chemical philoſophers, who ſo far from receiving it, take every method they can to ſuppreſs its inveſtigation. Though they have not publiſhed againſt it and abuſed it; yet their treatment has been more cowardly and ſhameful: they have allowed the reviewers, thoſe traders in criticiſm, to brand it with all the abuſe they can; while they never once offer publicly to inveſtigate it; well knowing, that to bring it to public notice, would be to eſtabliſh it: therefore they ſtand in the eye of juſtice, candour, and liberality, in the ſame predicament, as if they themſelves had ſo ſhamefully abuſed it,—indeed it would have been a more manly part. This kingdom is marked for its liberality and candour; but I am afraid we have our ariſtocrats in ſcience;—yea, even thoſe men who are bawling moſt for liberty, vulgarly called patriots: but the word implies a liberality and generoſity of ſentiment throughout.

As the forming fixed air into dephlogiſticated air, is certainly a proceſs by which we may accurately judge of the formation of dephlogiſticated air, I have paid a particular attention to it; but I will not give the minutiæ of the experiments, as many have done. If the public, after the uſage I have received, gets a detail from me, they may be ſatisfied. To other philoſophers the philoſophical Tranſactions are open for their long hiſtory of dry experiments: *but I hope the day of reckoning and retribution will come.* That fixed air, when ſoluted in water, will, by the action of the ſun, form pure air, has been proved by Dr. Prieſtley and others. As my theory ſuppoſes that fixed air and water, united to the rays of the ſun, will form pure or reſpirable air; therefore, to ſhew whether my theory or Mr. Lavoiſier's is juſt, I made a number of experiments. I took freſh diſtilled water, and expoſed it to a ſtrong heat, without getting any kind of air from it; I then added to it a quantity of fixed air, which it readily abſorbed, marking the quantity; after that, I expoſed it to the rays of a hot ſun. But as tranſparent bodies are well known to admit the rays of light to paſs through them, without arreſting or ſtopping their paſſage, I added an extraneous body; any will anſwer, ſilk thread, or a dead leaf, dried ſtraws, &c. which have an attraction for the air; and by this means I found a great quantity of pure air riſe to the top of the decanter, which contained the water; after that I expelled all the air from the water by heat, and I obtained rather a larger volume of pure air, than of the fixed air uſed; and not an atom of the latter. I found that two things were to be attended to in thoſe bodies, which were added to the water, to aſſiſt the proceſs; viz. they ought not to be tranſparent, and ought to have a kind of an electrical repulſion to water, ſo that the air may ſtand upon them in diſtinct globules: I found glaſs bodies, for theſe reaſons, improper, and likewiſe linen thread, which had a great attraction for the water.

It is remarked by philoſophers, that green animalcules appear when the water gives out air in the greateſt abundance. This appears to be from their arreſting the ſun's rays, and attracting the air in the water, the green ſeeming to be the beſt colour; and which nature indeed, makes uſe of in the great vegetable world, in attracting the rays of the ſun. When theſe green animalcules appear in the water, by gently adding fixed air, you may continue on, for a long time, the proceſs of generating pure air.

The globules of air, when they are ſeen ſtanding upon the ſilk, &c. appear little at firſt, but gradually grow bigger; the ſilk reflecting the rays, and by that means they enter the globule, warming it, and ſaturating the air, growing larger, and as it were generating or forming pure air, by the fixed air attracting the rays, and ſaturating itſelf, by neutralizing them along with the water. We ſee moſt of the ſaline productions of nature are formed of acids, an alkaline ſalt, (which I ſuppoſe fixed fire) and water, and forming regular cryſtals. In the vegetable kingdom, moſt of the bodies belonging to it are formed of acids, fire, water, and earth; in bitumens, oils, &c. a more concentrated fire is neceſſary with acids, fire, water, and earths; and the different concentrations, combinations, and proportions of theſe bodies make the different bodies upon the earth, without running into the wild ſpeculative opinion, that all bodies are formed of airs. No airs are formed of them.

But even our philoſophers have given very different opinions upon the origin of fixed air. Dr. Auſtin, whoſe experiments certainly deſerve the greateſt attention, formed fixed air, where there could not be the ſmalleſt reaſon of ſuppoſing there was any charcoal in the proceſs; ſo he, from theſe experiments, formed an hypotheſis that fixed air is formed from inflammable air, phlogiſticated air, and pure air. But theſe experiments of his are to be explained upon my ſyſtem. When pure air is fired with the pure inflammable air of metals, the fire is ſo intenſe as even to take from the acid its aerial ſtate, as fixed air, and forming it into a condenſed acid. But if pure air is fired in the heavy inflammable airs, from charcoal, oils, &c. the ignition will not be ſo intenſe, and the acid will ſtill keep its aerial form.—Now, here is the doctrine of fixed air, both by analyſis and ſyntheſis. If the nitrous acid is added to an alkali, or a calx, and expoſed to a ſtrong heat to aerilize them, they will form pure air; and if this pure air is burned in the heavy inflammable

mable air, the pure air will be formed into an aerial acid: but if fired with a purer inflammable air, the pure air will be formed into the *nitrous acid* it was made from. Nay, ſtill ſtronger, if this pure inflammable air is gently added to pure air, ſo that the ignition is not ſo intenſe, the acid of the pure air will ſtill retain its aerial form, viz. the aerial acid. This Dr. Prieſtley found, when he expoſed zinc and turbith minerial to a ſtrong heat.† And even

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† Dr. Bewley ſays, p. 65,—" With reſpect to the com-" buſtion of inflammable air, ſulphur, phoſphorus, &c. Mr. " Lavoiſier hath given ſome accurate experiments, which prove " the heat to be very conſiderable; but the heat, when fixed air " is produced, is conſiderably weakened in theſe combuſtions. " The difference is, indeed, very great; but it ſeems evident " that that muſt be owing to the intenſeneſs of the combuſtion, " and not to the particular ingredients burnt; as the ſame bodies " burning produce the different reſiduums. In burning inflam-" mable and oxygen gas, the fire is ſo intenſe, as to burn in-" ſtantaneouſly, going off with a loud exploſion; but when " theſe two bodies burn in a ſlow and gentle manner, they form " only fixed air. To prove this, Dr. Prieſtley has given many " experiments, and indeed, the proofs from them are very good " ones.

" But then, that his experiments may correſpond with his " very ſingular doctrines, he ſuppoſes that the fixed air is pro-" duced from the dephlogiſticated air, by imbibing inflammable " air in its naſcent ſtate, as he expreſſes it. To make the ex-" periment, expoſe zinc, iron, red precipitate, turbith mineral, " to fire in earthen retorts: and the proceſs is ſimply this; the " combuſtion takes place in a gentle manner, as the dephlogiſ-" ticated even thoſe inflammable airs which form pure air into the aerial acid, if they have a greater quantity of fire thrown into them by the electrical ſpark, fire, &c. ſo that in the combuſtion a more intenſe heat is produced, they will condenſe it

" ticated and inflammable airs are generating; for, as the heat " produces theſe airs, it will in conſequence ignite them. That " it is from this ſimple cauſe, is beyond all doubt; for, if you " take care not to raiſe the heat ſo high as to ignite the airs, " you will receive them both entire. Dr. Prieſtley was baffled " in many of theſe experiments, owing to their firing with an " exploſion, after a quantity had been generated: however, he " often produced them quite ſeparate; which he could not have " done, had there been (as modern chemiſts would have us be- " lieve) great attraction between the two airs. Now, theſe expe- " riments are in direct oppoſition to Mr. Lavoiſier's doctrine: for " here is fixed air generated, when water only ought to have " been generated; and there was poſitively no *carbone*.

" Nothing can more clearly eſtabliſh our hypotheſis, namely, " that the quality of the reſiduum, after burning theſe airs, de- " pends upon the intenſeneſs and degree of the combuſtion; for " if a column of theſe two airs goes off inſtantaneouſly, here all " the fire which theſe two airs poſſeſſed, acts at one and the " ſame time, and muſt produce a conſiderable quantity of heat. " But in thoſe proceſſes, where fixed air is formed, it burns in " a gentle and gradual manner. What ſhows this in a moſt " ſtriking light is, if the inflammable air from the wood is fired " with reſpirable air, it burns more gradually, ſo as to produce " fixed air. Yet, if this ſame inflammable air has a greater " quantity of fire thrown into it by the electric ſpark, or com- " mon fire, it will explode all at once, juſt as the inflammable " air from metals does, and produce the nitrous acid."

into the original nitrous acid. Nay, even the fixed air, which is formed by water and the ſun into pure air, and that fixed air, which is formed into pure air by expoſing the calx of mercury, (made from the corroſive ſublimate, decompounded by the fixed alkali) will, if fired with the inflammable air from metals, be found in the ſtate of the nitrous acid, as Mr. Cavendiſh found. I have found that thoſe inflammable airs, which will form pure air into fixed air, if they are mixed and paſs through a red-hot gun-barrel, will burn ſo intenſely, as to form the nitrous acid. The learned and ingenious Dr. Milner, by paſſing the volatile alkali through manganeſe, found nitrous air formed. As the proceſs was conducted in a red-hot gun-barrel, and as the manganeſe generated the pure air, it ſet fire to the volatile alkali, and the combuſtion was conducted ſo intenſely as to form the pure air into the nitrous acid and water; both of which acting upon the phlogiſton, either the unburned volatile alkali, or that of the iron of the gun-barrel, produced nitrous air. Can philoſophical chemiſts wiſh for more *clear and ſelf-evident demonſtrations?* They ought not to run into thoſe *wild conjectures* of water being formed of airs; and of charcoal and pure air, forming fixed air. Charcoal is a factitious body.

## OBSERVATIONS ON DR. BEDDOES' PAPER IN THE PHILOSOPHICAL TRANSACTIONS, ON THE PROCESS FOR CONVERTING CAST INTO MALLEABLE IRON.

DR. Beddoes has endeavoured to explain, upon the principles of the new theory, the phenomena of this proceſs for converting caſt into malleable iron, in two papers which are publiſhed in the Philoſophical Tranſactions. It is an explanation embarraſſed with difficulties; for, according to the principles of this theory, it is ſuppoſed that pure air is neceſſary to the proceſs, in uniting with the charcoal, and diſcharging themſelves as fixed air. But the Dr. ſays,—"Its action upon the "metal ſeems to be pernicious: I conſider its "preſence as an evil." Now, if Mr. Lavoiſier's theory be true, it would certainly greatly haſten the operation, by diſcharging the charcoal. But without following Dr. Beddoes in his experiments and obſervations, which being conducted under the influence of this theory, are contradictory and erroneous, I ſhall give the hiſtory of the phenomena, as I have obſerved them, explaining them upon my theory, and then we will ſee which more ſatisfactorily accounts for them.

I ſuppoſe that the caſt iron is only imperfectly metalized, and ſtill retains part of the charcoal uſed in the firſt operation. That upon heat being again applied, the ingredients begin to attract the fire of the charcoal; and, as they attract it, they give out the water they were united with: for moſt of the metals are found in the bowels of the earth in their ſtate of calces. I have found, that theſe calces are formed from the earth of the metal being united to the water, and not to pure air. My proceſs, in proving that, is (after carefully taking ſulphur and all extraneous bodies from them) by adding a concentrated acid to them, and then by expoſing them to a great heat, I get water from them.—See page 24.

Then, upon the water being ſeparated from the calx, will act as ſteam, from the influence of the fire, and form part of the charcoal into inflammable and fixed airs; the workman ſtirring the ingredients which aſſiſt the operation, that all parts may have the influence of the heat, and each particle come in contact with the charcoal. One very ſtriking phenomenon is, that a great heat will be generated, apparently from the ingredients themſelves, and not from the influence of the fire: and this, I have obſerved, is the time when the proceſs is moſt active, the calx receiving the charcoal's fire, and the iron becoming malleable. It is the ſame in the calx of gold, when it fulminates with the volatile alkali. The calces are then decompounding the alkali and charcoal; making the fixed fire of the alkali and charcoal become free fire, the calces taking or attracting it from its preſent chemical combination, uniting with it, and they becoming metals. But, in this operation, a great quantity of the fire becomes active, and eſcapes. This proceſs, as I have obſerved, is clearly ſeen in the calx of gold: the calx penetrating the volatile alkali, attracting its fixed fire from its combination with the alkali, and ſeizing a great part of it, becomes gold; while a great quantity of it eſcapes as actual fire, forming the exploſion.

In the proceſs of the iron, when the calx is attracting and ſetting free the charcoal's fire, there will be at that time the greateſt generation of airs: for the water of the calx of iron being expelled by the fire, it will be formed into ſteam, which will act upon the undecompounded charcoal, forming inflammable and fixed airs.†

Now, theſe phenomena are not to be accounted for by their theories; but mine gives a full and ſatisfactory explanation of them; indeed they are its ſtrongeſt evidence. And it explains a phenomenon which was always perfectly obſcure, how phlogiſtic bodies, (bodies which poſſeſs a great quantity of fixed fire) reduce metals. It evidently appears it is not from their uniting to the calces in their compound ſtate; nor in the ſtate which Stahl ſuppoſed, but in the ſtate of actual fire: and that this fire, at the time, is exceſſively intenſe, heating the whole maſs conſiderably. And it is a well aſcertained chemical fact, that a body, at the time of its diſunion from another body, which diſunion being by the influence of chemical attraction, will, from this chemical attraction, ruſh into the attracting body, and chemically unite itſelf to it, far more ſo than if the actual fire was applied from a common fire in an elaboratory.†

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† For I have found (in the proceſs of paſſing ſteam through charcoal) that when the charcoal was not expoſed to ſo ſtrong a heat, and the ſteam in the greateſt abundance, that the greateſt proportion of fixed air came over; and when great heat and leſs ſteam, more inflammable air.—See my Letter to Dr. Prieſtley and others.

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† Dr Beddoes mentions a phenomenon, which, I think, corroborates my opinion of the formation of inflammable and fixed airs from charcoal; that when the proceſs was almoſt finiſhed, and when there was little moiſture and great heat, there only came over inflammable air, and no fixed air comparatively.

Dr. Beddoes, in his book upon Mayow, ſays, p. 51,—" Now " let the pieces of iron be let down into the nitrous acid, by " looſening the cord, when a violent efferveſcence, accompanied " with heat, will ariſe, and the water within will be depreſſed " by the generated gas.—When the efferveſcence has continued " about twenty minutes, or rather when the water has been " depreſſed about three finger's breadth, the iron is to be raiſed " out of the liquor: you will now ſoon ſee the water within " aſcend gradually, and in an hour or two it will ſtand far above

the

There is a very ſtriking phenomenon, viz. when iron is hammered, it becomes red-hot, and takes fire. Now I do not ſee how the new theory can account for this fact; how hammering ſhould make it imbibe pure air. But, in driving a nail into the firmeſt wood, the phenomenon is the ſame, where the atmoſpheric air is excluded. The ſame phenomenon takes place if you rub two pieces of wood together, they will take fire; and, previous to their taking fire, they become charred. In this experiment, they ſuppoſe the pure air not neceſſary in charring; then why neceſſary in the

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" the mark; for having been at firſt depreſſed three finger's " breadth below, it will be now as much above the mark; ſo " that about one-fourth of the ſpace occupied by the air will be " filled with water: " *and indeed the water thus elevated will* " *deſcend, in no long time, to its former level.*" I ſuſpect ſome " miſtake here: the author ſays nothing more of this ſtrange new " depreſſion of the water: how can any air be generated in theſe " circumſtances? Did one of the pieces of iron at any time " come off the bunch, and remain behind in the acid? I wiſh " the paſſage were out of the book, or at leaſt, that ſome one " would explain it to me."

Now the explanation appears to be clearly this, upon taking the iron out of the nitrous acid by the cord, the iron was ſtill in a ſtate of generating more nitrous air; therefore, after the pure air was fully acted upon by the nitrous air already generated, taking the iron out of the acid at that part of the proceſs, its ſurface being covered with the acid, they go on generating more nitrous air; and in conſequence increaſe the volume of airs.

proceſs with the iron? I think there is no doubt, but both phenomena are upon the ſame principle. I rubbed two pieces of wood incloſed in a bladder, containing, in one experiment, azote, and in the other the aerial acid; the bladder being only one third filled with thoſe airs; ſo that I had liberty to hold each piece of ſtick ſo tight, as to give them the attrition neceſſary: and the ſame phenomenon took place, as if they had been rubbed in the open air: they became charred. If a piece of fir is expoſed to the influence of the dephlogiſticated marine acid air, it will be charred, the acid ſeizing upon the fir's phlogiſton; and, in the attrition, the wood likewiſe looſes its phlogiſton, eſcaping as actual fire. Now, in charring wood, every chemiſt muſt know the proceſs, and what comes from it, when charring, ſeen by placing the wood in a gun-barrel, and expoſing the barrel to a great heat. In the operation, a conſiderable quantity of inflammable air comes from it; and in iron the ſame. To char it, it is neceſſary to take away the inflammable air from it. Then, in theſe two proceſſes of attrition and the application of the dephlogiſticated marine acid, the wood muſt have loſt its inflammable air; which is really the caſe.

But let us attend particularly to the proceſs of inflaming iron, by taking from it its inflammable air or phlogiſton; or (to ſpeak with greater propriety) its fixed fire, by the power of mechanical friction.

I got a large piece of buckram, and glazed it, ſo that it was impervious to the air; I incloſed within it a ſmall ſmith's anvil, and a rod of iron; in one part of the cloth there was a hole, ſo as to admit a ſmith to put his arm into it, and then the hole was tied very cloſe upon his arm, ſo as to hinder the paſſage of any air, and the buckram was preſſed cloſe, ſo as to preſs out any air; then I filled it again with fixed air, paſſed through a heated gun-barrel, to exclude any moiſture, all the apparatus being extremely well dried for the experiment; after that I preſſed out all the air, in order that if any atmoſpherical air had been left, it might be expelled, and filled it with pure dry fixed air.* I deſired the ſmith to hammer the iron; taking hold of it with his left hand, through the canvaſs.

After he had ſtruck it for a long time, I carefully examined the rod, and particularly the particles that had been ſtruck from it; and I found that part of the rod which had been hammered, and the grains that had been ſeparated from it, had loſt, in a great meaſure, their malleability; to the eye they appeared nearly as if they had been calcined in an open fire. I found that the magnet that bore a dram weight of the iron, cut into ſimilar pieces, and incloſed in a piece of paper, would not bear above half a dram of thoſe that had been calcined in the experiment. That a diluted ſolution of the vitriolic acid produced nearly twice as much inflammable air, from that part of the iron rod which had not been hammered in the experiment, as from that which had; and that a ſmall quantity of fixed air came from the calcined iron. You will find the ſame reſult, if you employ in the experiment azote inſtead of fixed air.

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* There was an aſſiſtant to keep the ſides of the buckram from interfering with the hammer; and likewiſe a little window formed of glaſs, and glewed to the buckram, ſo as to aid the artiſt in ſtriking the rod of iron, which became very hot in the operation.

This experiment will be more eaſily performed, if you have a ſmall iron mortar, fixed in a ſmith's vice, and a very heavy peſtle, ſuch as the apothecaries uſe, with a bladder well dried, and filled with fixed air, and tied cloſe to the top of the ſides of the mortar, and round the lower part of the peſtle, ſo as to admit of the peſtle's motion, about four inches up and down, and pieces of iron being put into the mortar, and forcibly beat and ground for a long time.

Now here was a great heat generated, and a very ſenſible loſs of the iron's metallic principle, without either pure air or water being employed in the operation. I employed fixed air, as it is well known that it extinguiſhes red-hot iron immediately; that ſo far from conducing to its combuſtion, it has a ſtriking effect in extinguiſhing it.

An idea has been entertained, that fire is owing to the vibration of matter, and not to the action of a peculiar fluid. This was the opinion of Sir Iſaac Newton; he was no chemiſt; but had he traced fire in the different chemical proceſſes, he, no doubt, would have deſerted the opinion. I think with Bergman, that there cannot be a clearer fact, than that fire is produced from a certain material ſubſtance. He ſays, vol. I. p. 36,—"Numberleſs phenomena, which we cannot here "conſider, evince, beyond a doubt, that heat is "the effect of a certain material ſubſtance.—It is "ſufficient here to obſerve, that ſubtile matter of "heat occurs in two diſtinct ſtates; either it is at "liberty, in which ſtate it pervades all bodies, ſo "that all attain the ſame temperature, or it is "fixed by attraction, and does not exhibit ſenſible "heat, unleſs ſet free by more powerful attrac"tion." I think there is no philoſopher who has attended to chemical phenomena, but muſt be of the ſame opinion. But here I have given experiments, which prove, beyond a doubt, that fire is a certain material ſubſtance, and which material ſubſtance in the iron and wood was in the form of inflammable air; but, by the mechanical friction, was ſeparated from its chemical combination, as actual fire. We have many inſtances where mechanical compreſſion can ſet free actual fire. A very ſimple one is in Dr. Darwen's experiment on atmoſpherical air; by the compreſſion of it, he produced heat, and, by expanding it, he produced cold. I would have philoſophers attend to theſe experiments; I think they fully prove my ſyſtem of fire.

Dr. Fordyce's paper to the Royal Society, of heating the diaphragms of paſteboard and iron, only prove this, that metallic bodies, from receiving a certain quantity of fire, raiſe the temperature higher than ſome otherbodies do, by receiving the ſame quantity. This fact is moſt ſtrikingly proved in Boerhaave's famous experiment of mercury and water; the water raiſing the mixture ſo much higher than the mercury; though heated before the experiment to an equal degree. —The elucidation of which (ſee my former publications) is from the mercury (and the iron from the ſame cauſe) having already a high ſaturation of fire, (as phlogiſton) which fixed fire will act upon the actual fire, though fixed, raiſing the temperature; and when mercury, or iron, is cooled, they will give out leſs fire, becauſe part of this fire is fixed; but, nevertheleſs, this fixed fire will, in a ſmall degree, act upon the actual fire as actual. This, as we have juſt obſerved, is ſtrikingly ſeen in mercury; a pint of boiling water will raiſe a pint of mercury ſo much higher than it will do a pint of water of the ſame temperature with this mercury; but take away the mercury's fixed fire, or phlogiſton, by calcination, and the water will not heat it ſo much.

Steel is only iron, which has received a higher ſaturation of fixed fire, than iron. The proceſs of making it being in expoſing iron to phlogiſtic bodies; or in heating iron red-hot, and then haſtily extinguiſhing it in the cold water. The iron's fibres being diſtended with the heat, upon the mechanical application of the cold water, are ſuddenly contracted, and, in this contraction, fix the fire. The ſame as the fixed alkali, by being mechanically agitated with fixed air, will receive a greater ſaturation of it, than it before poſſeſſed, having a ſuperabundant ſaturation, as it were.

## ON THE FORMATION OF PURE AIR FROM MERCURY.

THAT famous experiment of Mr. Lavoiſier's, which is the pillar of their preſent hypotheſis, viz. in calcining mercury, there is pure air attracted by the calx, and upon its reduction (without addition) it gives out pure air; the explanation of which is given by Dr. Bewley.*

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* Dr. Bewley ſays, p. 92,—" The experiment which firſt " ſuggeſted, and is the foundation of Mr. Lavoiſier's doctrine, " appears

But in calcining mercury by agitation in water, I have found the mercury calcined, from the water alone, without any pure air. Dr. Bewley ſays, p. 104,—" Dr. Harrington hath clearly " proved

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" appears, upon the firſt glance, to be very much in favour of " it; and he ſays, to prove it both analytically and ſynthetically. " If mercury is calcined in atmoſpherical air, the air will loſe " its oxygen gas, being imbibed by the mercury; and, upon " reducing the mercury, a quantity of oxygen gas will come " from it; the mercury will be reduced, and the air become " reſpirable again. Now let us inveſtigate theſe phenomena, " and ſee whether Mr. Lavoiſier's theory, or that of Dr. " Harrington, will beſt account for them.

" Mr. Lavoiſier ſays, that the oxygen gas is a pure element. " Dr. Harrington ſays, atmoſpherical air is formed of fire, fixed " air, (or, as Mr. Bergman, with greater propriety calls it, the " aerial acid) and water; but the factitious oxygen gas of fire and " acid, water and earth. Now Mr. Lavoiſier allows that the " calcining of metals is an act of combuſtion, which is ſtrikingly " ſeen in the calcining of iron by burning. And Mr. Lavoiſier " muſt alſo allow that, when moſt bodies are burned in atmoſ- " pherical air, the oxygen gas turns to the aerial acid or fixed " air; and that, into whatever this oxygen gas is turned in the " burning of mercury, it is imbibed by the calx. I ſhould ex- " pect, that Mr. Lavoiſier will allow me to ſuppoſe, that the " combuſtion of one body is much the ſame as the combuſtion " of another, (juſt as the reſpiration of one animal is like to " that of another) that is, acts the ſame upon oxygen gas. " Then I will ſuppoſe that the burning of the mercury, and " moſt of the bodies, upon this earth is the ſame, and that they " all turn oxygen gas to an acid air. In one caſe only, the " oxygen gas is not imbibed by the burning body, but in the " other

“ proved the following facts ; namely, that the
“ calx of iron, made by ſteam, is formed by the
“ calx imbibing the pure water ; and that the calx
“ of iron, formed by burning it with the oxygen
“ gas, is from its imbibing the acid and water con-
“ tained

“ other it is. Then let us ſuppoſe that this fixed air, or aerial
“ acid, is imbibed by the calx of mercury. That the calx of
“ mercury has a great attraction for air, is a fact which has
“ been long known in chemiſtry. I will give Mr. Lavoiſier an
“ example. The corroſive ſublimate, of mercury, if united to
“ an alkaline ſalt, the *ſal abſinthii* for inſtance; the marine
“ acid will leave the calx of mercury and unite itſelf to the
“ alkali; and the fixed air of the alkali will unite itſelf to the
“ calx of mercury. This experiment is well known to chemiſts:
“ and it is equally as well known, that if this calx of mercury is
“ expoſed to heat, it will form oxygen gas, and the mercury
“ will be reduced. Dr. Harrington ſays in his Letter, p. 88.
“ But even fixed air may be formed into pure dephlogiſticated
“ air, if united to the calx of mercury (this calx we have all
“ along proved to have the greateſt attraction for concentrated
“ fire) viz. in the experiment of the corroſive ſublimate being
“ decompounded by an alkali; if expoſed to fire, it will yield
“ more empyreal air; but it cannot bear the explanation that
“ Mr. Kirwan gives of it, viz. the fixed air being decompounded;
“ for if the fire is not puſhed, you will get the dephlogiſticated
“ air from it, and the calx will not be reduced: nay, to ſhew
“ that it is not phlogiſticated after this proceſs, agreeably to Dr.
“ Prieſtley's own teſt, it will form with the nitrous acid, pure
“ dephlogiſtigated air again; or if united to the marine acid,
“ and then precipitated again with the alkali, it will form again
“ pure dephlogiſticated air. Our theory here appears ſo rational,
“ that

“ tained in the gas: but nothing but pure water “ is found in the calx made by ſteam. I have “ likewiſe found, that the calx of mercury, “ made in diſtilled water, by ſtrong agitation, (a “ mill,

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“ that, when this dephlogiſticated air is burned, it will be decompounded; that is, its fire will be ſeparated from the fixed air, “ both being produced in the proceſs of burning.”

“ Now, it is evident that Combuſtion, or the ſetting looſe “ a quantity of actual fire, has the power of turning the oxygen gas to an acid; and we muſt ſuppoſe that the aerial acid, when condeſended in the Mercury, muſt become “ concentrated, forming an acid of much greater acidity. “ That fire will turn the pure part of atmoſpherical air into “ fixed air, has been fully ſhewn in this treatiſe; viz. the electrical ſpark taken in atmoſpherical air; and it will alſo turn “ oxygen gas into the nitrous acid. It has likewiſe been proved, that when nature’s oxygen gas of the atmoſpherical air, “ and the factitious oxygen gas of chemiſts, are operated upon “ by pure fire, (ſee p. 84 of this Treatiſe) that they will be “ turned to the nitrous acid.

“ Then we are clearly brought to this concluſion, that acids “ and the calx of mercury produce oxygen gas; and what “ directly confirms this concluſion is, that if an acid, either of “ nitrous, vitriolic, or fixed air, is added to the clax of mercury, “ they produce oxygen gas.

“ Chemiſts ſay, that this production of oxygen gas, is from “ the oxygen gas, which they ſuppoſe the acids and the calx “ poſſeſs. But with reſpect to thoſe other bodies which are “ ſaid to poſſeſs oxygen gas, try if they will produce oxygen “ gas with the calx of mercury; and ſteam, as they ſay, is “ capable of decompoſition; then water (for inſtance) ſeems to “ be the moſt proper body, as containing the greateſt quantity “ of

“ mill, for inſtance, one of Dr. Prieſtley's experiments) will give out no oxygen gas, but pure water only, at its reduction, (when it is reduced without addition) and yet Dr. Prieſtley has “ proved

“ of oxygen gas: and likewiſe the calx of mercury has a ſtrong attraction for its inflammable air. Water then, conformably to them, ſhould be the moſt proper for the calces to get oxygen gas from: and therefore the propereſt body to unite to the calx of mercury: but it will not anſwer to unite the calx to this or to any other body, but only to acids.—Moreover, what ſtrikes directly againſt their hypotheſis is, the factitious oxygen gas is fully proved by Dr. Harrington, to be a different kind of gas, from the pure part of atmoſpherical air.

“ Then, in this caſe, we muſt be allowed to ſay, that the bodies, neceſſary to produce oxygen gas, are an acid, the calx of mercury and water. It is well known that acids and the earth of metals have a ſtrong attraction for each other; and theſe two bodies have a ſtrong attraction for fire or phlogiſtion. The nitrous acid, by being expoſed to a great heat, becomes red and phlogiſticated; and the calx of mercury has ſo ſtrong an attraction for fire, that heat alone will reduce it. And, as to phlogiſton, the nitrous acid has ſo very ſtrong an attraction for it, that chemiſts have placed theſe two at the head of the table of attractions; and the calx of mercury greatly attracts phlogiſton and becomes reduced. It is no leſs true that heat will aerilize both the nitrous acid and the mercury.

“ Now, under a review of all thoſe circumſtances, are we not authorized to ſuppoſe that, when theſe bodies, the acid, the calx and water, are for a long time expoſed to a great heat, that they will fix a quantity of this heat or fire, and be aerilized with it, forming that neutral phlogiſticated body, called oxy-

“ gen

" proved it to be a calx, viz. the *precipitate per ſe.*
" See Prieſtley, vol. IV. And I have alſo found,
" that, at its reduction, it imbibes inflammable air,
" and is a calx ſimilar to that made from iron by
" water."

" gen gas? The action of the fire implies this; for when they " become red, they give out only empyreal air: and it is very " well known that fire or phlogiſton reddens the nitrous acid.

" That the acid gets neutralized with the ſire and water, " appears from Mr. Cavendiſh's experiments. This proceſs is " ſtill more evident in lead. If lead is burned in a quick way in " atmoſpherical air, it will form only the grey calx of lead, as " it imbibes the acid of the air in its reduction: and if the calx " is expoſed to a great heat, it will give back again only that " acid. But if this lead is calcined in a gentle way, by being " expoſed to the reverberation of the heat, it becomes gradually " red, till at laſt the red is highly florid, and of the ſame colour " as the calx of mercury.

" That the calces become red from the fixed fire in them, is " ſtrongly confirmed by this, that their colour is ſimilar to that " which the blood receives in the lungs; this has been fully " proved by Dr. Harrington, to proceed from no other cauſe, " but receiving fire or phlogiſton: and, what farther corroborates " this opinion is, that alkaline ſalts, whichI hope, I may now " be allowed to call fixed fire, will produce the very ſame effect " upon the blood, forming it into that high florid red colour; " and oils the ſame.

" That part of the mercury goes along with the acid, in " forming the oxygen gas, hath been fully proved by Dr. " Prieſtley: this is ſtill farther confirmed by nitre. Nitre is " that body beſt adapted to form oxygen gas, an ounce produ- " cing half an ounce of oxygen gas. Now, alkaline ſalts are " found to be already fire fixed and concentrated by nature; " therefore

" water." I ſee, from the Analytical Review, that upon the continent they are making ſimilar experiments. " The reviewer of Crell's Chemical Journal in the June Chronicle, having noticed an experiment by Mr. Gren, in which he received quickſilver calcined *per ſe*, without obtaining dephlogiſticated air, and requeſted him to repeat it, as, if confirmed, it would ſtrike at the root of the new theory. Mr. Gren informs him, that it has been done more than once, by Mr. Weſtrumb, whoſe letter on the ſubject he tranſmitted to him. Mr. Weſtrumb put half an ounce of quickſiver, calcined *per ſe*, into a ſmall retort, with a neck three feet long: to this he luted a right angled tube, that terminated in a glaſs with two apertures,

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" therefore part of the proceſs is already executed, the fire being " already fixed: conſequently, the acid and the alkali only want " fire enough to aerilize them.

" What then muſt we think of that hypotheſis, which ſup- " poſes that a ſtrong acid and an alkali can be ſeparated after " they are united, and they ſay, that by a ſuppoſed ſeparation, and " a ſuppoſed attraction, the acid is decompounded of its oxygen " gas and phlogiſticated air. But where is their proofs that " alkalies have any attraction for phlogiſticated air? They have " none. Nay, the moſt whimſical (or ſhall I call it abſurd) part " of all is, (as Dr. Harrington has ſhewn, ſee p. 17 of this " Treatiſe) that Dr. Prieſtley got ſuch a quantity of airs from " nitre; that in one proceſs the alkali muſt have attracted the " acid's dephlogiſticated air, and its phlogiſticated air in the " other proceſs; and in both caſes, both airs being nearly above " the weight of the acid: but enough of ſuch abſurdities."

tures, by means of which it was connected with the pneumatic apparatus. The lute conſiſted of gypſum ſpread on linen, over which ſeveral ſtrips of linen, ſmeared with a mixture of quick-lime, and curds were wrapped. The retort being placed in a crucible, and ſurrounded with ſand, was expoſed to the heat of a good wind furnace. It was ſcarcely red-hot, when drops of clear water appeared in the neck of the retort: theſe gradually increaſed, and collected in the glaſs. They were followed by quickſilver, in its running form, without a ſingle bubble of air making its appearance. Mr. Weſtrumb has made experiments on the ſubject in different ways, with calcined quickſilver and phoſphorus, with the former and ſulphur, and in other manners, and the reſults are altogether contradictory to the modern ſyſtem of the French chemiſts. Mr. Gren is now preparing the black calx of quickſilver, having incloſed a pound of quickſilver in a veſſel faſtened to the hammer of a fulling mill for the purpoſe, in order to make ſimilar experiments with that."—Vid. Analytical Review, for Auguſt, 1792, p. 473.

I have found, that if you expoſe red lead, immediately after it is made, to a ſtrong heat, you can get no oxygen gas from it. But, by being expoſed to the air, ſo as to receive moiſture from it, it will then give out oxygen gas at its reduction, as before it wanted the water neceſſary for its aerial ſtate. And to prove that it was from that cauſe that it would not give out any pure air is, if you expoſe it to a ſtrong fire in that ſtate, (in which it wants water) you will reduce it, (without addition) but no pure air will come from it.

Now, it certainly is impoſſible to reconcile theſe facts to Mr. Lavoiſier's ſyſtem. The calx of lead ought not to become metallic, without parting with pure air: as this body is one from which he got pure air; indeed, it and the calx of mercury being the greateſt pillars of his theory: for if you take the other calces of lead, the grey, &c. they will only give out fixed air at their reduction. They having not been expoſed to the fire of a reverbatory furnace, to ſaturate the acid, (which it received from the air in its calcination) which is neceſſary to the formation of pure air. I am happy in ſeeing thoſe experiments proſecuted on the continent, as they directly ſtrike at the root of the new doctrine. My experiments are not attended to; but when they come from others, ſuch as Mr. Weſtrumb, they are favourably received;—*truth muſt prevail.*†

ON

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† It has been found that the pure air, made from the calx of mercury and an acid, when breathed, poſſeſſes a quantity of mercury, ſo as to produce a ſalivation; though the air had been well waſhed in water previous to its being breathed. Therefore, I think, this confirms my opinion, that part of the calx goes

## ON ALKALIES BEING COMPOUNDED OF FIXED FIRE.

Mr. Fourcroy found that alkalies, when united to the dephlogiſticated marine acid, are capable of being ignited, the ſame as any combuſtible body.——" When M. Fourcroy poured the " concentrated ſulphuric acid on the oxygenated " muriat of potaſh, he obſerved a violent effer- " veſcence, and the production of a white vapour, " which, though it reſembled in ſmell the oxyge- " nated muriatic acid, had a character peculiar " to itſelf: the ſalt and the acid both became of " an orange colour. He then varied the experi- " ment, by caſting ſome of the muriat into the " acid: the efferveſcence thus occaſioned, was " ſcarcely perceptible: but, when the mixture " was ſtirred with a glaſs tube, a violent exploſion " took place, accompanied with tranſient flaſhes of " red light; after this commotion had ſubſided, a " ſecond agitation produced another exploſion not " leſs violent, and accompanied with more ſplen- " did corruſcations than the former. On bringing

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goes to the formation of the air. The breathing of this air in conſumptions, particularly thoſe which have been ſuppoſed to have originated from a venereal taint, I ſhould ſuppoſe would be advantageous. And likwiſe venereal patients, whoſe conſtitutions, from irritability, would not bear mercury any other way.

" the flame of a taper near ſome of the mixture, " which had already made one exploſion, white " vapour aroſe from it in great abundance, and a " detonation took place, which broke the veſſel " that contained it, and was ſucceeded by a num- " ber of partial exploſions of the parts of the " mixture which were diſperſed to a conſiderable " diſtance. A piece of phoſphorus, immerſed in " this vapour, took fire, and occaſioned a detona- " tion ſtill more violent. A mixture of this " muriat with the concentrated nitric acid, pro- " duced ſimilar phenomena in a yet greater de- " gree.

" Theſe phenomena M. Fourcroy recommends " to the attention of philoſophical chemiſts; he " aſcribes them to the ſudden and ſimultaneous " ſeparation of light, condenſed vital air, and " oxygenated muriatic acid, from the muriat."— See the Appendix to the Monthly Review, for Auguſt 1792, p. 514.†

Now let us attend to the experiment upon the muriat of potaſh being expoſed to the influence of the concentration of vitriolic and nitrous acid, which, having a ſtronger attraction for the alkali, expels the dephlogiſticated marine acid. But in this expulſion it is united to a part of the alkali;

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† What makes me anxious in quoting thoſe *liberal* gentlemen, the reviewers, is, that I found them miſconſtrue my obſervations ſo ſhamefully: now they certainly cannot contradict their own publications.

and the heat generated in the proceſs aſſiſts the dephlogiſtcated marine acid in penetrating and letting looſe the fixed fire of which the alkali is formed.* When men have eſtabliſhed any hypotheſis, they account for phenomena, without adverting whether it is conſiſtent with reaſon or not.

Let us conſider Mr. Fourcroy's explanation.—" He aſcribes them to the ſudden ſimultaneous ſeparation of light, *condenſed* vital air, and oxygenated muriatic acid from the muriat." The muriat is formed from the calx of lead, and, as they ſay, from the acid imbibing the pure air of the lead. Now in the *condenſation* of the air, a great quantity of light and fire is produced; ſeen in the combuſtion of iron. And, ſtrange! in its *expanſion* it produces the ſame fire and heat, they ſay. This is directly againſt the firſt principles of their ſyſtem, viz. bodies giving out fire upon their condenſation, and imbibing it upon expanſion: But theſe contradictions are of *no moment to them.* ——Does not the *phoſphorus* produce the ſame *effect*, and is it not a *combuſtible body?* Are we to loſe ſight of every former *rational fact* of our forefathers? As the phoſphorus produced the ſame effect as the alkali with the dephlogiſticated marine acid; can we have a doubt but that they both act as inflammable bodies? If our modern chemiſts are ſo credulous, let them examine the ingredients after the operation, (taking care to avoid from the exploſion a waſte of the ingredients) and they will find, both a part of the phoſphorus and alkali have diſappeared. And if the phoſphorus is imagined to have imbibed the ſuppoſed pure air of the acid, they do not ſuppoſe the alkali to have any attraction for it; but they equally produce combuſtion. Here is a violent combuſtion, with a great ſeparation of heat and light: and, from theſe *wonderful theories*, the acids are ſaid to be the combuſtible bodies containing the fire; while the alkali and phoſphorus are not taken into the account. How *the doctrine of our fathers is mangled!* But let me tell chemiſts, that the fixed alkalies will become volatile;* and that the volatile alkali is combuſtible,

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* That alkalies are formed of fixed fire, is fully proved by Dr. Bewley; and that they are formed into fire by the combuſtion of gun-powder, &c.

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* Mr. Scheele found that the common oils would become volatile oils, by the fire from lime, becoming ſoluble in ſpirits of wine. See p. 175, Experiments on Air and Fire.

Dr. Bewley ſays, p. 111,—“ The acetite of potaſh, as “ Mr. Lavoiſier calls it, is a neutral ſalt formed of the acetous “ acid and the fixed alkali. Now, it is well known, that this “ ſalt, (as Mr. Lavoiſier obſerves, p. 270) will give out ammo- “ niac in diſtillation; and by ammoniac he means the volatile “ alkali. Then, can there be a ſtronger proof of the truth of “ our hypotheſis, that an acid, when united to other bodies, as “ ſalts, earths, &c. concentrates a great quantity of fire, ſo as to “ change the fixed to the volatile alkali, as in this proceſs of the “ acetite of potaſh? And it alſo clearly aſcertains this fact, that “ by an addition of fixed fire, the fixed alkali will become vola- “ tile, buſtible, we well know. Mr. Scheele ſays, that if you throw the cauſtic volatile alkali into a hot crucible, it will burn with violence; and it will likewiſe form inflammable air, and explode.

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The formation of nitre, both to the philoſopher and the ſtate, hath long been a *deſideratum:* it will produce the greateſt quantity of pure air; and is the

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" tile. And it proves, which is a fact of ſtill greater conſequence, that when an acid and an alkali are expoſed to heat, they will concentrate a very great quantity of fire: therefore the nitrous acid, which is an acid of a ſtronger attraction for the alkali, and for fixed fire, will, (as we have ſhewn) in the diſtillation of nitre, attract and concentrate a quantity of fire, and form oxygen gas. All theſe are fair and clear deductions from unqueſtionable facts.

" With this review, we may be able to account for all the phenomena of the late numerous experiments made by chemiſts for theſe twenty years; and I aver, that there is not one of theſe phenomena, but may receive an eaſy and rational explanation from this hypotheſis. When oxygen gas is formed from acids and earths, we have ſuppoſed that the acid, with fixed fire, and a little of the earth, are aerilized into the neutral aerial ſalt or nitre, called oxygen gas. That there is a ſmall quantity of earth, as much as is neceſſary to combine the fixed fire, the acid and water together, is very probable: for (as Mr. Beaume obſerves) it has all along been ſuppoſed, by old chemiſts, to be a kind of baſis to ſalts; and theſe bodies, I ſhould think, form both oils and ſalts. That all bodies, which contain a great quantity of fixed fire, are formed of acids, water, and earths, is what Dr. Harrington has " always the principal ingredient in gun-powder. I had collected a number of experiments and facts, relating to its formation, ſome years ago, and meant to have had them publiſhed in the Philoſophical Tranſactions: but this avenue to fame I found ſhut againſt my diſcoveries. I have had the audacity to contradict the opinions of ſome of its firſt members, and in conſequence I muſt be *run down*: but as the phenomena elucidate theſe Eſſays, I ſhall give the heads.

Nitre is formed from the putrefaction of animal and vegetable ſubſtances: but it muſt be conduct-

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" always taught, nay I will venture to ſay, proved. But he " proves that the pure air of the atmoſphere is not formed of " earth, or the ſtrong mineral acids, but of water and a weak " acid, called the aerial acid, or fixed air; and therefore an air " very different from the factitious oxygen gas: but that the " gas may contain a little earth, when made from metals, is " very probable. Dr. Harrington ſays in his Letter, p. 135, " As our theory ſuppoſes that there is a ſmall quantity of earth " in the artificial empyreal air, to aſcertain this, I examined, " very accurately, the reſiduum, after decompounding the empyreal air by the electric fluid; and I always found a depoſition of an earthly ſediment. That the quickſilver could not " produce it, I aſcertained, by putting in a column of diſtilled " water between it and the air; ſuch a quantity as would dilute " the acid, ſo as that it could not in the leaſt act upon the " mercury. Beſides, if the proceſs is conducted over ſoap-leas, " or lime water, by a chemical examination, you will equally " find that there has been an earthy depoſition from the air's " being decompounded." And Dr. Prieſtley has brought a " number of experiments to prove, that nitrous air contains a " little earth, and how difficult it is to detect it."

ed under the influence of a warm ſun, and the ingredients piled together in neither too large a maſs nor too ſmall, in order to make the putrefaction as intenſe as poſſible; ſo that putrefaction ſhall be ſo highly conducted as to decompound the fixed air into the nitrous acid, the ſame as has already been done in intenſe combuſtion.—— And likewiſe, when putrefaction is ſo intenſely conducted, all the phlogiſton, of the putrid maſs, will be decompounded into free fire; but this free fire, meeting with a proper baſis in the ingredients, will form a ſaturation of fire, as an alkaline ſalt. We need not wonder at this, ſince, in burning vegetables, &c. a ſimilar ſaturation will be formed. —That lime, from old buildings, will produce nitre, particularly in hot countries. And as thoſe buildings being expoſed for a long time to animal effluvia, heat, &c. will have the putrefactive proceſs regularly and ſlowly conducted by a ſtill conſtant ſucceſſion of freſh animal matter and heat; ſo that, in a long time, the fixed air is broke down, or changed into the nitrous acid. The nitrous acid, I ſuppoſe, generated in thoſe proceſſes, where acids are changed under a great degree of caloric. That all acids may be generated into each other I think is a very clear fact.

When I was upon my experiments in changing fixed air into pure air by the ſun and water, I forgot to mention that the ſpring water I made uſe of contained an earth, held in ſolution by fixed air, ſo that, after a long expoſure to the ſun, the earth was precipitated. The fixed air uniting to the rays of the ſun and water, formed pure air: but it requires a long expoſure before this proceſs takes place.†

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† And likewiſe that if vegetable bodies are placed in water, and expoſed to the ſun; as the putrefaction of the vegetable takes place, it will give out a quantity of fixed air, and this fixed air, by the action of the ſun, will be formed into pure air. Now any of theſe experiments concerning fixed air, which I have mentioned in theſe Eſſays, is ſufficiently clear to eſtabliſh my theory. But I will finiſh my obſervations and arguments upon it with this fact: the illuſtrious Bergman, Dr. Prieſtley, and many others, have found that, in paſſing the electric ſpark through common air, fixed air is formed. Now in this experiment the free fire of the electric fire is to that degree of high temperature, that it ſets free the atmoſpheric fire; and the electric fire is to that intenſity, that it will diſſolve iron in its quick and inſtantaneous paſſage through it. I ſhould be willing to reſt my theory upon this experiment alone; for to endeavour to explain it upon any other theory than mine, is ridiculous.

Dr. Prieſtley ſays, vol. III. p. 299, "Mr. Metherie found, "p. 146, though nitrous air is obtained from a ſolution of "mercury in nitrous acid, *almoſt all the acid* is found in the ſo-"lution." And Dr. Fordyce found *almoſt all the acid* in the ſolution of zinc in the vitriolic acid. Then can we have a doubt but both ſolutions are from the ſame cauſe; and not ſuppoſe the one from a decompoſition of the water, and the other from a decompoſition of the acid. The nitrous acid has a ſtrong attraction for metals, the ſame as it has for calcareous earths; and if I add as much of the acid as to neutralize the earth and metals, and no more; and, if upon examination of them after the ſatu-

ration,

All the trials which Dr. Pearſon put his ſuppoſed charcoal too, prove clearly that it was a ſpecies of ſoot, as the common ſoot will juſt produce the ſame phenomena. But need I hint to the chemical world other more neceſſary trials; paſſing the ſteam of water through it, imbibing air when heated, &c. &c. &c. But indeed I do not know but ſoot will ſtand theſe trials; for the ſuppoſition of its being charcoal is ſo abſurd, that I have not ſpent much time upon it. I have regularly followed our philoſophers in detecting their errors: but my patience is nearly exhauſted. I would adviſe them to *caſt about*, as the hunter expreſſes it; for the longer they are in doing it, the more *awkward* muſt their behaviour appear.

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ration, I find all the acid, only allowing a little for what the nitrous air took up from the metal; can chemiſts form the moſt vague conjecture that the nitrous acid is decompouuded in the metallic *ſaturation*, as all the acid is found entire, and the ſame in both the ſaturations? But ſtill more forcibly to contradict ſo very *abſurd* an opinion; by paſſing the electric ſpark through nitrous air, Dr. Van Morum reduced three-fourths of it into the nitrous acid.—(See a full explanation of this in my Letter, p. 32.) What led to this *extraordinary hypotheſis*, was the experiments of Mr. Lavoiſier upon mercury, and which I have ſhewn can likewiſe *only* be accounted for by my hypotheſis. See page 56 of theſe Eſſays.

Dr. Prieſtley, Mr. Kirwan, Mr. Cavendiſh, and others, who uſed once to make ſo great a figure in aerial philoſophy, are now perfectly mute. To what muſt we impute it ?—Time will unravel all things.—*But hitherto they have had neither the liberality to contend nor acknowledge.*

# APPENDIX.

## OBSERVATIONS UPON THE REVIEWERS OF LITERARY PRODUCTIONS.

*Gentlemen of the Monthly Review,*

YOUR behaviour (from its complexion, the candid reader would call malicious) has been ſuch, that in juſtice to myſelf I can continue no longer ſilent. You have been profuſe in your criticiſms upon my chemical publications; but hitherto I thought them not worthy of notice; nor would I have adverted to them, even now, were it not, that in the ſtatement of my doctrines to the public, you are guilty of the moſt wilful perverſion. From *the influence you are under*, I knew your criticiſms would be ſevere, but I always thought they would be liberal and juſt; that you would not deſcend to the meanneſs of miſrepreſenting my principles, or give a wrong turn to my mode of reaſoning. When I firſt gave to the public my theory upon air, I endeavoured to prove, that the air is not phlogiſticated in the act of reſpiration, but imparts its fixed fire to the blood; and that phlogiſton is not an elementary body, but fixed fire; which I proved, by a variety of experiments, and that the air is formed of fixed fire, water, and the aerial acid. You, the monthly reviewers, have negligently, yet arrogantly, diſcuſſed this doctrine and treated me with the moſt ſupercilious contempt. The whole of your criticiſms are made up of pompous declamation; nothing of argument, ſave only a curious anſwer to my arguments and experiments upon putrefaction. And here I would beg leave to quote from Dr. Bewley's Treatiſe, p. 197, as apropos to my preſent purpoſe, ſome powerful arguments I have advanced in proof of my doctrine.—"Dr. Harrington's ſyſtem of airs did "not originate from the experiments of gun-barrels, &c. but "from an accurate obſervation of nature. Take an egg, and "examine its fluids; they are mild and bland, conſiſting of "a watery *mucus*; but expoſe it to the air, and it will become "highly putrid: its fluids are changed from a pure ſtate to one "highly alkaleſcent and noxious, emitting a moſt nauſeous "ſtench. I need not enlarge upon this topic, as every one "muſt be acquainted with the phlogiſtic alkaleſcent ſtate of its "putrid fluids. Then, how muſt the egg have received all this "alkaleſcency? From the air, no doubt, as there was no other "body that acted upon it. The air, according to their opinion, "is highly phlogiſticated; then the wonder ſtill increaſes, as "we have not only the phlogiſton of the putrid egg, but alſo "the phlogiſton which the air has received, to account for. "According to Dr. Prieſtley's explanation, one egg will phlogiſticate two thouſand gallons of air; therefore, agreeably to "his theory, this egg muſt have given to the air one thouſand "gallons of inflammable air; or, according to Lavoiſier, ſome "ounces of charcoal; or, to others, an immenſe quantity of "phlogiſton.

"My reader muſt excuſe me, if I cannot bring myſelf to "believe, that this fine, mild, bland lymph could poſſeſs ſo "much phlogiſton, or charcoal, as not only to turn it into a "ſtate ſo highly putrid and offenſive, but even to phlogiſticate "ſuch a quantity of pure air. And I beg leave to diſſent from "the opinion of thoſe chemiſts who believe it; their reaſons and "chemiſtry being ſo very different from mine. To convince "their judgment that they are wrong, is perhaps not in my "power; yet I hope, I ſhall be able to convince their ſtomachs. "Let thoſe who hold the opinion I am combating, firſt ſwallow "a ſound egg, and then a putrid one, and I am ſure their "ſtomachs will be convinced, and of courſe their heads; the

" ſympathy and relation between theſe two parts of the body, " being ſo near and intimate: after this doſe they would, I " think, be of my opinion. Mr. Lavoiſier is ſo much ſtruck " with the ſtate of putreſcency, that he wonders that chemiſts " have not been more attentive to it: ſome chemiſts have not " paſſed it by; for, if he will pleaſe to attend to what Dr. " Harrington ſays in his publication in 1781, he will there ſee " the proceſs of putrefaction fully demonſtrated and proved.— " In the putrefaction of vinegar, the acid is turned alkaleſcent. " See Dr. Harrington."

Now, Mr. Reviewer, let us diſcuſs this great argument of yours, as it is the only one in your firſt criticiſm that you have brought againſt me, to prove that you are juſtified in your *damnation* of my ſyſtem. It is certainly a moſt intereſting fact, which philoſophers have not attended to, viz. the great change that animal bodies undergo, when they putrefy. But here this Reviewer inſtantly explains it: he ſays, brimſtone, when it is expoſed to heat, is equally as offenſive, and ſtinks. Could any critic, particulary one that fills the honourable chair of the Monthly Review, have made uſe of ſuch a one. The ſulphur, by being expoſed to the fire, I readily agree with you, *ſtinks*: but is it changed in its qualities from the expoſure? is it not perfectly the ſame ſulphur, or does it injure the air? But I muſt refer you to Dr. Bewley's *expedient* of ſwallowing the eggs. Mr. Reviewer, that you could ſuppoſe there is any analogy betwixt the brimſtone and egg--What muſt a poor author ſuffer, attacked by ſuch an unmerciful critic, with damnation in one hand and brimſtone in the other! Dr. W—— ought to have wielded leſs deſtructive weapons. A *Chriſtian divine* ought to have fought with more Chriſtian-like arms. *They are only fit for the devil himſelf.*

In your next review you ſay I hobble after Dr. Prieſtley.— If directly to attack Dr. Prieſtley, in ſaying, that what he calls dephlogiſticated air is phlogiſticated, and what he calls phlogiſticated air is dephlogiſticated,—that the act of reſpiration is giving phlogiſton to the blood, and not receiving it. Now the Dr. Mr. Critic, has given up his firſt idea, that the great principle of reſpiration is in receiving phlogiſton from the blood. And he has likewiſe followed me in his opinion that all airs have water for their baſis,—and that phlogiſton is of an alkaline principle;—and in his laſt paper upon the firing of inflammable and pure airs, I have ſhewn in theſe eſſays how he has followed me. And theſe are all the points in which we now agree.

I, in my Letter, addreſſed to Dr. Prieſtley, &c. ſay, that, agreeably to the late doctrines, the compoſition of water and gun-powder is nearly ſimilar. For nitre may be principally formed into dephlogiſticated, and charcoal into inflammable air; the two bodies which form water. Theſe are my words, page 2,—" But let us enquire, 1ſt, That inflammable air (or " phlogiſton) and dephlogiſticated air form water.—Of all " the ſingular changes effected by a chemical proceſs, this is " the moſt extraordinary. That an alkali and an acid body " would unite and form a neutral body, partaking of neither, " has long been known to chemiſts; but that charcoal ſhould be " formed into inflammable air, and nitre principally into dephlo- " giſticated air, leaving a reſiduum; not quite one half of the nitre, " which Dr. Prieſtley found to contain no nitrous acid, but an " alkaline baſis, (ſee Dr. Prieſtley, vol. IV. p. 295) and that " theſe ſhould form water.

" 2d. If we are acquainted with the chemical bodies which " compoſe any compound one, we can make that body. Thus " ſulphur is formed of the vitriolic acid and phlogiſton, and nitre " of the nitrous acid and an alkali. The common vitriol (ſo " abundant in nature) of the vitriolic acid and iron: and we " can form all theſe bodies in chemiſtry.

" Then, agreeably to this rule, we ſhould, by mixing charcoal " with nitre, directly make the ſame body as water; however, " by adding the reſiduum left after making dephlogiſticated air, " (which,

" (which Dr. Prieſtley found to have an alkaline baſis, and not
" half the weight of the original nitre) to a proper quantity of
" water, they would be exactly the ſame; at leaſt formed of the
" ſame materials as charcoal and nitre."

The monthly reviewer gives this anſwer, vol. lxxx. p 339,—
" The author purſues this *curious* thought a little further; but,
" inſtead of following him, let us try if it will apply to his own
" hypotheſis. Empyreal air, or vital air, according to him,
" conſiſts of phlogiſton, water, acid, and earth. Charcoal,
" he admits, is phlogiſton;* and to throw every poſſible ad-
" vantage on his ſide, we ſhall take the acid in the very ſtate in
" which he affirms it to exiſt in empyreal air, that is, in the
" ſtate of fixed air. If, therefore, we acidulate water with fixed
" air, and add to it ſome powdered charcoal and earth:† this
" compoſition, on his own principles, ought to be the very
" ſame thing with vital air."

But is Dr. Harrington's doctrine to be overthrown by a turn of witticiſm? By attending to this mighty critic, we may ſee that he has ſhamefully and wilfully perverted the truth. The hypotheſis, adopted by me above theſe twenty years, and made public above twelve, is, that pure air is formed of fire, fixed air, and water; and that when the fire is fixed and neutralized with the fixed air and water, it may be called phlogiſton, ſince phlogiſton is only fixed fire.

Speaking of charcoal in the ſame Letter, I ſay again, p. 26,
" The theory is ſimply this: vegetable bodies are principally
" formed of the vegetable acid, with water, and an earthy baſis;
" which is nature's own compound to attract the fire of the
" ſun, concentrating it. When it is expoſed to the fire, the
" fire is concentrated, or attracted into the compound, forming
" charcoal; and when the heat is puſhed further, there is ſuch

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* A direct and intentional error; for in the ſame book I expreſsly make charcoal a factitious body, and the ſame through all my publications.

† I ſuppoſe no earth in atmoſpherical air.

" a high

" a high concentration, as to form inflammable air; but if an " over proportion of water enters into the compound, a conſi- " derable leſs quantity of fire enters, and they form fixed air."

In your review, you might as well have ſaid that your friend *ſulphur* ſhould be added to the fixed air and water inſtead of charcoal. But in both theſe bodies the fire is fixed and neutralized with other bodies.—It is thus, Mr. Critic, that you ſhamefully pervert my theory, and miſlead the judgment of your readers. I agree with you, that to produce or not, by proper experiments, pure air from fixed air, water, and fire, would be a fair trial of the truth or falſeneſs of my theory. The rays of the ſun are the pureſt fire, and theſe, I ſuppoſe, nature makes uſe of. If, therefore, theſe bodies act upon each other, *they will make the pureſt air*, as is evident from well known experiments, which I have mentioned in theſe Eſſays.—See p. 40.

The other part of your criticiſm, in which you endeavour to depreciate the juſtneſs of ſome of my experiments, without trying them, is ſo much of a piece with the reſt of your behaviour to me, that I do not think it merits a reply; I ſhall therefore leave you to *your own reflections*.

You have honoured Dr. Bewley with ſimilar treatment, and your critical remarks upon his Treatiſe are of the ſame complexion. He brings a charge againſt ſome chemiſts,—a ſerious one it is, and not eaſily got over,—that they have adopted part of Dr. H.'s ſyſtem, without being ſo honourable as to acknowledge it. Our great reviewer makes uſe of all his rhetoric in their defence, Dr. Bewley ſays in a note, that Mr. De Luc adopted Dr. Harrington's idea, in ſuppoſing that air injured by reſpiration, &c. is purified in the clouds, and that fire is a body capable of being chemically attracted. The ingenious author, in the Medical Spectator, p. 153, ſays, " Chemical philoſophers " have now pretty generally ceaſed to conſider heat as a quality; " they have begun to call it the *matter of heat*. Dr. Robert " Harrington, ſo long ſince as the year 1781, has not only con-

" ſidered it in this light, but hath ſuggeſted that, like other " objects of chemiſtry, it has its affinities and elective attrac- " tions."

I compared its attractions to be the ſame as that of alkalies and acids, not laying latent, a word Dr. Johnſon thus defines; hidden, concealed, ſecret. But it is chemically attracted and united to the fixed air and water, neutralizing them ſimilar to the neutral ſalts. Then, Mr. Reviewer, when you ſpeak of the fire which neutralizes the vitriolic and the phlogiſtic acids into ſulphur and phoſphorus, will you call their fire latent; or is that which I prove neutral ſalts to poſſeſs, to be called latent heat? Dr. Black, in his very important diſcovery of heat diſappearing in vapour, &c. gives no chemical idea what became of it; but that it laid dormant: and ſo far was he from ſuppoſing it was chemically attracted like other chemical bodies forming phlogiſton, that he thought phlogiſton was a pure element, as Dr. Stahl did, and not actual fire, chemically neutralized with other bodies.

Our very liberal reviewer does not indeed deny, that Mr. De Luc had adopted the idea after me; but takes no notice of the ſecond charge, viz. an opinion that the air is a homogeneous fluid, and Mr. De Luc's adopting it from me; only, he ſays, our theories differ, which to be ſure they do; and this Dr. Bewley expreſsly mentions. But what Dr. Bewley aſſerts is this, that Mr. De Luc had adopted Dr. Harrington's idea of the clouds being nature's elaboratory, as he ſignificantly expreſſes it. And, Mr. Reviewer, even this diſcovery is certainly a great philoſophical object. But I have ſhewn, not only where air is formed, but alſo its qualities and compoſition. This Mr. De Luc does not even pretend to have done.

But, Mr. Reviewer, I may ſay, without breach of good manners, that in every ſtatement of my ſyſtem, you make wrong aſſertions, on purpoſe to turn it into ridicule; and your motive in doing ſo, no doubt, is, that the learned world may take your word, and diſregard it. What a pity it is that ſuch illiberal critics ſhould have ſo much influence in the literary world!—According to you, I aſſert, "that fire is an element, or ſimple "ſubſtance, moſt abundant in the *upper* regions of the atmoſ-"phere." I ſay no ſuch thing; but, on the contrary, that the greateſt cold abounds there. But this I ſay, that bodies in the *upper* regions muſt firſt come in contact with the rays of the ſun, and that thoſe rays which are arreſted and chemically united to the impure air and water, will become fixed, and ſo add no *actual* heat to that part of the atmoſphere.

You ſeem to triumph in the ſuppoſition, that fixed air and water, being heavy bodies, will not aſcend to the upper regions. I thought none conſidered it a diſputable point, that the clouds contain a great quantity of water. I readily grant, that fixed air is ſpecifically heavier than atmoſpherical air; but you muſt alſo grant me, that fixed air makes a part of the air that has been breathed; and Dr. Prieſtley found that airs of different ſpecific gravities did not ſeparate agreeably to their gravities, but ſtill retained their ſituation with reſpect to each other.—And it is generally allowed, that expired air is lighter than air that has not been breathed, and aſcends in the atmoſphere. Upon this idea it was, (as I have obſerved) that Mr. Montgolfier formed his balloon. If two birds are placed in a cylinder, cloſed at each end, the bird in the lower end will live longer than that in the upper. But if this does not carry conviction home to you, allow me to mention a fact, which, I ſhould think, ought to have greater weight with you than any conception you can boaſt of. The gentleman who aſcended mount Blanc, found that lime-water became immediately turbid upon the ſummit of that mountain.

But I have ſhewn in my Thoughts on Air, (ſee p. 297) that air, rendered impure by breathing, becomes again pure, if expoſed to the ſun; therefore I drew this concluſion, that this change will be produced in any part of the atmoſphere. Hence, Mr. Critic,

you

you may make this expired air heavier or lighter, juſt as you pleaſe.

As my theory is gaining ground, you thought you could not be off giving ſome account of it, and though you have often reviewed it, you had never done it before. But pray, why do you not endeavour to relieve the other gentlemen from the charges laid againſt them? From your ſilence we may rationally infer, that you think the taſk too difficult.

You affect to be witty upon Dr. Bewley's appeal in his dedication to the Royal Society. Whatever you may think, others are of opinion, that, as a body, or as individuals, they ſhould do ſomething; and as to an *appeal* to the Houſe of Commons, I thought you had not been ſo *fond* of it.

You are extremely kind in taking ſo much pains to draw up a connected view of the whole theory, though I have but a mean opinion of your *ability* and *candour* for ſuch a taſk.—What you have done, is done moſt *admirably*. I cannot give you credit for ignorance; no, it muſt have been with deſign, that you have ſo wilfully perverted Dr. Bewley's meaning. This is upon a par with the reſt of your criticiſms. You ſay in the Monthly Review for 1791, p. 438,—"The gentleman would, "perhaps, be more likely to gain his point, by drawing up a "connected view of his whole ſyſtem, detached from the refu-"tations, criminations, and other heterogeneous matters, which "envelope and obſcure it. Philoſophers could then underſtand "and examine it with facility; they could judge how far it is, "or is not, ſupported by known facts, or by ſuch new facts as "the author might lay before them; and they could ſee where "its great ſtrength or its weakneſs lies. We had attempted "ſomething of this kind ourſelves, and had beſtowed no ſmall "labour on it: but we found it moſt prudent to deſiſt, leſt we "ſhould really miſunderſtand ſome of the author's doctrines, or "be thought to have wilfully miſrepreſented them. A ſhort "example will explain our meaning.

“ Fire, when free and uncombined, is, in the Harringtonian “ language, *actual fire* or *heat* : when united with bodies by “ affinity, it is called *fixed fire* or *phlogiſton*, and it cannot be ſet “ free but by the aſſiſtance of *acid*. Atmoſpheric air conſiſts “ of *a mild acid* ; namely, the aerial acid or fixed air ; a mild “ concentration of *fixed fire* ; and *water*. In virtue of its *acid* “ and *fire*, it enters into true *combuſtion* on the application of “ actual fire : but its own concentration of fire being *mild*, the “ heat produced is not intenſe, and the combuſtion is limited ; “ if it had a *high* concentration of fire, combuſtion, once begun, “ would ſpread till the whole atmoſphere was decompounded. “ Combuſtible bodies have a *high* concentration of fire, and the “ atmoſphere only furniſhes *acid* by which their fire is ſet free. “ Nitre is analogous in compoſition to air ; for it conſiſts of an “ *acid* and an alkali, and the alkali conſiſts of *fixed fire* and “ *water* ; and therefore nitre ſupplies the place of air in “ combuſtion.

“ Now, if the only office of air in combuſtion be to furniſh “ *acid*, we cannot underſtand why any air ſhould be neceſſary “ for the combuſtion of bodies that abound with acid. We “ thought at firſt, that the acid in theſe bodies might be united “ with their fire in a *neutral* ſtate, and thereby rendered *inactive* : but, the author aſſures us, that, in the air itſelf, the “ acid is *neutralized* by the fire, and yet that air is combuſtible “ *per ſe*, without the contact of any phlogiſtic ſubſtance ; ſo “ that (though he happens to be miſtaken in the fact) the “ *neutralization* of the acid, according to him, can be no impediment to its agency in combuſtion. Again, that the “ aerial acid, or *fixed air*, a ſubſtance ſo remarkable for its property of *extinguiſhing* combuſtion, ſhould be the primary “ agent in *producing* and *ſupporting* combuſtion, appears to *us* “ unaccountable : but this acute philoſopher has, doubtleſs, contrived means of counteracting its deleterious power, though “ all our attention has not been able to diſcover in his book any

“ intimation of ſuch a corrective, nor of any other principle “ exiſting in the atmoſphere, than *fixed air*, *fire*, and *water*.”

A moſt cunning Jeſuitical review this is of my theory. But let Dr. Bewley ſpeak for himſelf; this is the only way to ſhew how groſsly the reviewer has *perverted his doctrine*. Dr. Bewley ſays, p. 39,—“ Nature has wiſely ordained, that the atmoſpherical “ air, a light aerial phlogiſtic body, ſhould be the agent, by “ which combuſtion is conducted, as being eaſily ſuſceptible of “ taking fire. But then its fire is ſo light and weak, that it is “ not capable of conſuming or burning, without the aid of “ another combuſtible body, which is ſet on fire by the atmoſpherical fire being kindled; and then the ſtrong combuſtible “ body, (as a candle for inſtance) is alſo lighted, both fires acting together, ſo as to keep up the combuſtion. But were “ fire applied to the candle, without the agency of atmoſpherical air, no combuſtion would follow, even ſuppoſe the candle “ is compoſed of inflammable materials. For both atmoſpherical air and a combuſtible body are required to produce ignition, and that upon account of the attraction and concentration “ of fire. There will be no combuſtion, unleſs they both act “ at the ſame time; or, that intenſe heat neceſſary to ſupport “ the ignition, or the conſuming of the body, will not be produced. The cauſe of which is moſt probably this; the “ intenſe heat produced acts upon the combuſtible bodies, concentrated fire, or phlogiſton, ſo as ſuddenly to expand them, “ and to break their attraction from the bodies to which they “ were chemically united. For the conſumption of one particle “ of its fire, is the means of conſuming the neighbouring particles, and ſo on, till the whole body is conſumed, or broken “ down by the ſeparation of its concentrated fire or phlogiſton, “ and from the mechanical expanſion, a conſequence produced “ from great heat; ſo that the whole texture of the body will “ be broken and reduced to aſhes. This is evident from intenſe “ heat, conſuming or burning bodies without ignition; viz.

" light combuſtible bodies, as paper, which, being expoſed to a " great heat, will be equally conſumed or burnt, as if it had " been ignited. But that it is not ignited, is evident from the " air not being acted upon by the paper: and the ſame phe" nomenon will take place in foul air, or in *vacuo*. Hence it is " evident that the paper is conſumed by having its phlogiſton " or fire ſeparated from it by the heat."

Here it appears that the air's fire is firſt ſet free, which fire then acts upon the fire in combuſtible bodies, and ſets it free alſo, and ſo combuſtion takes place. But if the air's acid is of ſervice to combuſtion, it muſt be by aſſiſting in ſetting free the fixed fire of the air, with which it is neutralized; and not by its acid acting upon the combuſtible body. But if you, Mr. Reviewer, like not this doctrine of the fixed air of the atmoſphere contributing to the combuſtion, you may even let it alone, as it alters not my theory: which is this, that pure air, as well as the candle, is a combuſtible body; that the air is neceſſary to the combuſtion, from its fire being ſo eaſily ſet looſe; and therefore it acts upon the candle's fixed fire.—But not, Mr. Reviewer, from its acid, viz. fixed air acting upon the fixed fire of the candle, but only upon the fixed fire with which it is connected, in the ſtate of pure air.

And, as Dr. Bewley has ſhewn, what farther corroborates this theory is; eſſential oils formed into vapour will not promote the combuſtion of the candle. But if theſe eſſential oils are formed into a vapour or air with the nitrous acid, the dephlogiſticated nitrous air of Dr. Prieſtley, and which I have clearly proved to be formed of the nitrous acid and phlogiſton; then this air will allow a candle to burn in it, the ſame as pure air. Here the acid is neceſſary to ſet looſe the fixed fire of the eſſential oils to which it is united. But I will quote one of your paragraphs in the Appendix to the Monthly Review, for 1792, p. 513,—" It was formerly ſuppoſed that all thoſe elaſtic fluids, " which are unfit for reſpiration, were equally improper for com" buſtion: pyrophorus, however, has been found to burn in

" nitrous gas with greater violence than in atmoſpheric air; and " it appears, from the experiments here related, that the oxygenated muriatic acid gas, or the dephlogiſticated marine acid of " Scheele, forms another exception to a notion once ſo generally " received. A wax-taper immerſed in this gas continued to " burn; the flame, indeed, became longer and ſmaller, and " aſſumed a reddiſh hue like that of a torch ſeen through a " miſt: but it was obſerved that the wax burned faſter, and " that the wick was ſooner conſumed, than in common air: " ſimilar phenomena occurred on repeating the experiment with " a lamp; the flame was red and gloomy, ſurrounded with a " denſe vapour, and the carbonic ſubſtance of the oil ſeemed to " be ſeparated with greater rapidity than uſual, and to be " whirled in a kind of torrent around the wick. The phoſphorized hydrogen, or phoſphoric gas of Gengembre, on coming " into contact with the oxygenated muriatic acid gas, immediately took fire, and burned with a deflagration not leſs violent " than in the atmoſphere, but with a flame leſs bright than it " yields in vital air. The ſulphurated hydrogen, or hepatic " gas, on the contrary, exhibited no inflammability on being " thus mixed."

I do not ſay that atmoſpherical " air is combuſtible *per ſe*." This is ſlyly aſſerting, without any qualification, that, agreeably to my doctrine, atmoſpherical air is combuſtible *per ſe*.—I only ſay that Dr. Prieſtley formed dephlogiſticated nitrous air, ſo highly combuſtible as to explode of itſelf when fire was put to it. In the year 1785, I proved, in a manner ſo evident, that this dephlogiſticated nitrous air is formed of phlogiſton and the nitrous acid, that thoſe who attend to my proofs cannot poſſibly miſtake them. Now, *your wonder* that fixed air ſhould be the primary agent in combuſtion, is, no doubt, as great a *wonder* to me. Therefore, Mr. Reviewer, your learned criticiſms are founded upon *falſe data* and *obſervation*. I would have expected ſomething more *acute*, at leaſt more *juſt*.

But you have not, in this your tremendous review, taken any notice of Dr. Bewley's general doctrines, nor of his obſervations upon Dr. Crawford, Lavoiſier, Cavendiſh, Kirwan, &c. By all means keep down the ſpirit of inquiry, when ſuch reſearches are not to your taſte. It would have been better not to have entered upon any inveſtigation, but to have pronounced my *damnation* at once. *I have now anſwered all your mighty objections, or criticiſms.*

Mr. Reviewer, I ſhall *hint* to you that you ſpeak too confidently of Dr. Pearſon's experiments on phoſphorus, as clearly evincing the formation of charcoal. But a critic who undertakes to decide and direct the public upon theſe very abſtruſe and intereſting ſubjects, ought to have ſome knowledge of the ſubject. Now, Mr. Critic, there are many experiments where this black ſooty matter has been produced, and where there can be no ſuſpicion of charcoal forming any part of the proceſs: yea, *even* where ſo far from fixed air being *decompounded* into charcoal in the proceſs, *it* is even *generated.* And, Mr. Critic, I will give you your *friend* Dr. Prieſtley's experiments to prove what I ſay, as you will not certainly diſpute what he ſays. He ſays, vol. VI. p. 116,—"Willing to try the effect of heating "iron, and other ſubſtances, in all the different kinds of air, "without any particular expectation, I found that iron melted "more readily in *vitriolic acid* air than in dephlogiſticated air, "the air was diminiſhed as rapidly, and the inſide of the veſſel "was covered with a *black ſooty matter*, which, when expoſed to "heat, readily ſublimed in the form of a white vapour, and left "the glaſs quite clean. The iron, after the experiment, was "quite brittle, and muſt, I preſume, be the ſame thing with "iron that is *ſulphurated;* but I did not particularly examine "it. Of ſeven ounce meaſures of vitriolic acid air, in one of "theſe experiments, not more than three-tenths of an ounce "meaſure remained; of this two-thirds was *fixed air*, and the "reſiduum of this was inflammable. I had put three of ſuch "reſiduums together, in order to make the experiment with "the greater certainty." Now, Mr. Critic, I need not name to you, that this air is the vitriolic acid aerialized; and that the vitriolic acid will, along with iron, produce inflammable air. But when applied under an intenſe degree of heat, this inflammable air is expelled in the ſtate of this black *ſooty matter*. The iron, after the operation, is partly calcined, and quite brittle.

And further, to hammer it more into you, and to oblige you to open your eyes, how unwilling ſoever; the vitriolic acid air will expel the black *ſooty matter from phoſphorus*. Dr. Prieſtley's and Dr. Pearſon's experiments are the ſame; the phoſphorus has its fixed fire expelled by an acid; the acid being in one proceſs the *vitriolic acid air*, and in the other the *aerial-acid air*: and what the fixed air, or aerial acid wanted in ſtrength, is compenſated by the great heat Dr. Pearſon employed in his proceſs of decompounding the phoſphorus.

Dr. Prieſtley ſays, vol. II. p. 12,—"A piece of *phoſphorus* "remained a day and two nights in vitriolic acid air, without "ſenſibly affecting it. It gave no light in this air; but the "upper ſurface of it turned black, and the ſurface of the "quickſilver on which it lay, had a deep yellow or blackiſh kind "of ſcum upon it, as if it had been in part diſſolved by the acid." Now, Mr. ſuperlative Critic, the ſteam of water will expel inflammable air equally from phoſphorus and iron; and it appears, when expelled under a great degree of heat, and little water, not to give the fixed fire an aerial form. Both in Dr. Prieſtley's and Dr. Pearſon's experiments, your two friends, it takes the form of a black ſoot: yes, Mr. Reviewer, even from a letter in your own review, phoſphorus, by paſſing through a glaſs tube, under a great degree of heat, will depoſite this black ſooty matter upon the glaſs. And this ſame correſpondent formed this black ſooty matter from phoſphorus and lime; the latter not efferveſcing in the leaſt with acids, therefore, Mr. Critic, *could poſſeſs no fixed air*, whatever your great *authority* may ſay to the contrary.

That the vegetable acid in wood has a great attraction for fire, is ſeen in charring: it attracts ſuch a quantity, that when it meets with the ſteam of water, it is aerialized either as the aerial acid or inflammable air, the proportion of each air depending, as I have before ſhewn, upon the quantity of water uſed, and the degree to which the wood is charred. To prove that the aerial acid has a ſtrong attraction to concentrate fire; if it is expoſed to the common fire, with water, it will receive a concentration, ſo as to form azote; but if expoſed to the rays of the ſun, receiving a greater proportion, it will form pure air: but if ſtill greater, applied in the powerful ſtate of electrical fire, it will form inflammable air. A well-known experiment.—And to prove what a powerful ſtate of fire the electrical ſtate is. If you paſs this electrical fire through the nitrous acid, the acid will receive ſuch a proportion of the fire, as to form a nitrous air that will admit of a candle burning in it, with an enlarged flame, juſt the ſame as Dr. Prieſtley's nitrous air formed from the nitrous acid and zinc, improperly called by him dephlogiſticated nitrous air: it ought rather to be called phlogiſticated; and if the electrical fire is paſſed through the phoſphoric acid, it will, like the aerial acid, receive ſuch a quantity as to form inflammable air.* Now theſe are *well-known* experiments, Mr. Critic; which, I ſuppoſe, you are a ſtranger to.—*Can we have a more regular connected chain, without running into thoſe wild theories of the decompoſition of water and the decompoſition of acids: and likewiſe deſtroying all our former rudiments, and advancing a thouſand other abſurdities.*

I mean theſe *hints* to you and your friends; and that the public may have a *fair repreſentation.*

Gentlemen of the *Critical Review*, I refer *you* to the Medical Spectator Extraordinary: his charges are very *heavy* upon your criticiſm of my ſyſtem. To give a little ſpecimen, I ſhall quote a few lines: he ſays, p. 20,—"Dr. Harrington appeals "to the experiments, which, for a conſiderable time, were ſuppoſed to demonſtrate that WATER may be decompounded, "and that it conſiſts of inflammable and *de*phlogiſticated airs.— "He aſſerts, on the contrary, that theſe airs are themſelves decompounded; and that WATER, as one of the component "parts, inſtead of being *made*, is only depoſited or precipitated. Does the critic deny the force of this argument? No "—He admits its truth, but ſurreptitiouſly attempts to give "the reputation of it to others; and even lays claim himſelf to "the ſame idea in reſpect to inflammable air—he tells us the "concluſion was ſo obvious, that he himſelf made it long before "he even heard of Dr. Harrington's attempts, and inſidiouſly "refers his readers to the 65th volume of his Review, which "was publiſhed in 1788, eight years after Dr. Harrington firſt "publiſhed his general theory, that WATER is a conſtituent "part of atmoſpherical air, and three years after he had in the "moſt expreſs terms extended the idea to every ſpecies of factitious air."

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* That theſe inflammable airs are not from a decompoſition of water is evident; for in paſſing the electrical ſpark through water, in all forms and ſituations our *ingenious* chemiſts can invent, no inflammable air can be obtained. But to ſhew its formation; if you paſs it through the cauſtic volatile alkali, you will generate it in great abundance.

As to you, the gentlemen of the *Analytical* Review, you make no *analyſis*, you only *declaim;* and have the boldneſs publicly and openly to avow this *ariſtocracy* in theſe inveſtigations. What a profanation, for any one to offer to meddle with their reſearches! Though they ſhould ſet the Thames, nay even the ocean, on fire, dare any impious tongue cry "hold! hold!" There is the Grand Turk in government, and the pope in religion; but I never heard of ariſtocrats in ſcience: yes but there are, and who have their janiſſaries too, (thoſe reviewers) to ſtrangle a poor author at their nod.

In their reviews of my general ſyſtem, they have not made any attack upon my general principles, but only upon ſome particular and very ſecondary parts, which they thought they could criticiſe, or exert their witticiſms upon; like to a general who never faces the main body of the enemy, but lies in ambuſh for a foraging party, or any ſmall detachment, and cowardly attacks them, and puts them all to the ſword, and then publiſhes a great and important victory to the world: but ſometimes on their return to their camp, from their *ſuppoſed* victory, they, in their turn, get themſelves cut to pieces. Such has been the conduct of thoſe liberal gentlemen, the reviewers, and I hope ſuch will be their *fate*.

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I have been informed Dr. Prieſtley has been very anxious in his enquiries to know who is the author of a Treatiſe on Air, ſubſcribed Dr. Bewley. I acknowledge myſelf to be the author; and how far I am juſtified in taking a fictitious name, I leave to the candour of the public; my motives being theſe. I need not name how much my feelings have been hurt by theſe *cruel* and *undeſerved* criticiſms of the reviewers for theſe thirteen years; obſerving that my enemies, without coming boldly to the conteſt, *ſkulked*, allowing the reviewers thus to abuſe it; thereby, hoping the public would, without *inveſtigation*, condemn it. And I found that my name being annexed to any arguments, however forcible, would not be attended to. I acknowledge I was induced to put a fictitious name to my book. Any man who has experienced thoſe very abuſive criticiſms in the monthly publications, will eaſily admit of my apology, in adopting a ſcheme which opened a proſpect of avoiding ſuch heavy abuſe. Nay, I hope the day is not far off, when, inſtead of condemning me, the world will ſee the truth of my doctrines, and the ſeverity of my uſage, and turn the reproach upon their own heads, as being more deſerving of it. Living in a place where theſe deep aerial inveſtigations are not attended to, my publications are not peruſed by any one, before they go to the preſs; and my mind being ſo intent upon the abſtruſe parts, could not attend ſo well to thoſe ſecondary minutiæ. Thoſe who are not converſant in the ſubject, but only take their opinion from my *good friends, the reviewers*, it cannot be ſuppoſed I ſhould conſult; therefore I have been left entirely to myſelf. Indeed I wrote to one gentlemen, whoſe judgment I could depend upon, viz. the author of the Medical Spectator, ſaying that I had a book ready for the preſs, and aſking his opinion of the propriety of a fictitious name, and he agreed with me; but, upon mature conſideration, wrote to me not to do it. Having then printed many of the ſheets, I did not chuſe to reprint them. That I had any deſign of deceiving the public, by taking upon me the name of Bewley, *I directly deny.* The reaſons why I hit npon that particular name were, that I wrote the book principally at a country houſe in the neighbourhood of Carliſle, contiguous to which their lived a farmer, whoſe name was Bewley; which name I thought an obſcure one, and principally confined to the north. That there was a gentleman of that name, who had, in the Appendixes to Dr. Prieſtley's volumes, wrote a paper or two upon the acidity of fixed air, *after* Bergman, and one upon the nitrous acid being contained in the atmoſphere: but he had been dead for many years. His name was Mr. William Bewly,—I took the name of Dr. Richard Bewley.* Now I ſuppoſe that no man could conceive that the dead could publiſh: beſides, after I had my book in the preſs, his name ſtruck me; therefore, to obviate any miſtake, I, on purpoſe, quoted him as another author; and I was certain my good friends, the reviewers, would take care to have the miſtake rectified, if there ſhould be any. But theſe are hard reflections, to think, after my labour and diſcoveries, I ſhould be under the neceſſity of taking the name of Bewley, or any other, in order to engage the attention of the public: but the day of retribution will come, and then we ſhall ſee whether the reviewers or I are more worthy of condemnation—*They are not above the tribunal of the public. I have given a juſt* REVIEW *of their conduct to me.*

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* And to ſhew that I took the name from the farmer, and not from the chemiſt, I have ſpelt the name as it is done by the former, (uſual in the north) and not as the latter.